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THE RÔLE OF DETERMINING TENDENCIES IN LEARNING

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The purpose of the present study is to discover the effect of instructions upon the problem of learning. Stated concretely, it seeks an answer to the following questions: Does an attitude or purpose make for more efficient functioning of the individual? Specifically, does an active 'set' of the organism make for more rapid learning than a passive 'set'? Can a determining tendency, established by instructions, increase learning and recall? Does the 'set' of an individual play a part in the perceptive activity of the individual?

In the experiments reported here the determining tendencies were secured through definite verbal suggestions. This method was employed in order to control as many variables as possible.

The experiments were conducted in the Psychological Laboratory at the University of Oklahoma during the years of 1933 and 1934. A total of 673 *Ss*, students in elementary psychology, were used. Six separate experiments were made. Both objective records and verbal reports were obtained from all the *Ss* in every experiment. The study includes both a quantitative and qualitative approach to the rôle of determining tendencies in learning.

SENSE MATERIAL

Experiment I. Experiment I was devised to discover the rôle of determining tendencies in the learning and reproduction of sense material. Do 'sets' function in the recall of ideas? Do they condition the experiential field and its organization?

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Observers. In this experiment there were three groups of *Os*: A, the motivated group; B, the counting group; and C, the reading group.

Material. The material for learning, which was used by all groups, was a prose selection, known as the "Marble Statue." This standardized paragraph, consisting of 166 words and 67 ideas, was borrowed from Whipple.[1]

Procedure. The *Os* were tested a group at a time. At the beginning of the test-period a copy of the material to be learned was given every *O*.

(1) Group A, set for ideas and tested for ideas, were given the following instructions:

I am going to pass out a sheet of paper containing typed material. Please keep typed side down until instructed to read it. The typed material is a brief story. I want you to read the story with the intention of remembering as many ideas or facts as possible. You will have 2 min. Try as hard as you can to remember the ideas in the story for you will be tested for them.

At the ready signal the *Os* turned the page and studied the story for 2 min., as measured by a stop watch. As soon as the papers were passed in, the following supplementary instructions were read:

Write the story of the 'Marble Statue,' or reproduce the paragraph, including as many ideas as you remember.

After sufficient time was given for the reproduction of the story, the following instructions were read:

Write what you were doing during the 2 min. you were reading the paragraph with the intention of remembering the ideas.

(2) Group B, set to count words and tested for ideas, were given the following instructions:

I am going to pass out some sheets of paper containing typed material. Please keep typed side down until instructed to read it. The sheet contains a paragraph of typed material. I want you to count the words on the page. You will have 2 min.

At the ready signal, the *Os* turned the page and counted the words for a period of 2 min. At the termination of this period the following instructions were given them.

Write the story of the 'Marble Statue,' or reproduce the paragraph, including as many ideas as you remember.

When the *Os* had reproduced the story the following instructions were read:

Write what you were doing during the 2 min. you were counting the words in the paragraph.

(3) Group C, set to read and tested for ideas, were instructed as follows:

I am going to pass out some sheets of paper containing typed material. Please keep typed side of paper down until instructed to turn it over. I want you to read the paragraph.

The *Os* were allowed 2 min. to read the material and then the following instructions were read to them:

Write the story of the 'Marble Statue,' or reproduce the paragraph, including as many ideas as you remember.

As soon as the *Os* had completed the reproduction of the story, the following instructions were read:

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[1] G. M. Whipple, *Manual of Mental and Physical Traits,* 2, 1915, 209.

Write what you were doing during the 2 min. you were reading the paragraph.

The introspective reports of the *Os* were used as a check upon the objective data, and they also afforded a basis for a more detailed account of determining tendencies in learning situations.

The papers from the *Os* of all groups were graded on the basis of the number of ideas reproduced. Every correct idea counted as one point. A score of 67 was perfect.

Results. Table I shows the means, the differences of the means, and the standard errors of the means of the three groups. Group A, motivated to learn the ideas and tested for recall of the ideas, reproduced on the average 51.1 ideas. Group B, instructed to count the words in the same material and tested for recall of ideas, reproduced on the average only 7 ideas. Group

TABLE I

COMPARISON OF MEANS UNDER CONDITIONS OF DETERMINATION AND NON-DETERMINATION

Experiment	'Set' in learning and test same		'Set' in learning and test different		Diff.	σ diff.	Diff. / σ diff.
	Group	Av.	Group	Av.			
I	A	51.10	B	7.00	44.10	2.63	16.76
Sense material	A	51.10	C	37.95	13.15	2.50	5.25
II							
Sense material	A	58.52	B	14.20	44.32	2.08	21.30
III	A	6.54	B	4.26	2.28	0.72	3.16
Nonsense syllables	A	6.54	C	3.89	2.65	0.75	3.52
IV							
Geometrical forms	C_0	4.70	A	2.80	1.90	0.45	4.22
and colors	C_f	6.34	B	4.42	1.92	0.35	5.48
V							
Pictures and	C_o	7.06	A	3.90	3.16	0.39	8.10
colors	C_p	6.90	B	5.03	1.86	0.34	5.47

C, instructed to read the material, reproduced on the average 37.95 ideas. The difference between the averages of Group A and Group B is 44.10, σ 2.63 with a critical ratio (diff./ σ diff.) of 16.76; and between Group A and Group C, 13.15, σ 2.50 with a critical ratio of 5.25.

The results demonstrate that Group A will practically always recall more ideas than will Group B; that Group A will recall on the average more ideas than will Group C. The objective data indicate that determining tendencies, aroused in the organism through instructions, do influence the learning of meaningful material.

Verbal reports. The verbal reports afford additional insight into the relation of determining tendencies to logical learning. A few random samples from each group are given.

Mc: "I read the paragraph, not just to get the story, but to get the ideas in it."
Ab: "I tried to get the details arranged in my mind so that I could remember them."

A strong set was aroused in the *O*s to remember the ideas, and around this determining tendency the ideas in the material were organized into a total pattern.

The *O*s of Group B organized the perceptual material around a determining tendency to count the words. This determination to count words was so strong that for many of the *O*s, although they perceived the words, the words as such had no meaning. Thirteen out of the 27 *O*s in this group did not recall a single idea, as the following sample reports show:

Da: "I paid no attention to anything but counting the words."
Wi: "I counted the number of words paying no attention whatever to the sense of the material."
Co: "I did not read a single word of the paragraph, or see any that I remember."

The set in these cases was so potent that printed words as such had no meaning. Around the determining tendency was organized numbers, not meanings of words or sentences.

The reports from Group C, instructed to read the material, are conflicting. Various sorts of sets were developed as the following excerpts show:

Ke: "I was merely reading the story, not trying to remember the incidents in the story."
Me: "I did not attempt to remember details."
Mc: "I reasoned that some sort of reproduction of the material would be asked for; therefore, I tried to remember the details."

The data indicate that determining tendencies influence learning. The sets of the organism condition the items which integrate into a total pattern. Purposes may be developed, and these do influence the experiential field and its organization.

Experiment II. Experiment II was planned to demonstrate further the function of determining tendencies in the learning of sense material. May potent determining tendencies be aroused in individuals so that their learning is more rapid and efficient?

Observers. In this experiment two groups of *O*s were used: A, the motivated group; and B, the distracted group. Group A contained 46 *O*s, and B, 57.

Material. The learning material employed in this experiment was a paragraph of prose, called "The Dutch Homestead." The paragraph, standardized and consisting of 180 words and 94 ideas, was also taken from Whipple.[2]

Procedure. To Group A, set to learn ideas and tested for ideas, highly motivating instructions were read. The members of the group were informed that they would be tested for the ideas in the selection. The instructions given in detail in Experiment

[2] Whipple, *op. cit.*, 210.

I serve as a general pattern for the instructions in all the following experiments with the exception of Experiment VI. The *Os* in Group B, set to read under distraction and tested for ideas, were merely instructed to read the material and at the same time to follow the oral reading by an assistant of a selection from Garrett[3] on the researches of the Russian scientist, Pavlov. Each group read the same material for a period of 2½ min.

Methods. The test papers of the *Os* were graded on the basis of the number of the ideas correctly reproduced. Every correct idea was given a count of one. The highest possible score for any individual was 94. The verbal reports gave a more detailed insight into the relation of determining tendency and learning.

Results. Table I shows that Group A, when aroused by a prepotent determining tendency, recalled on the average 58.52 ideas, while Group B, when set to read the same material under distraction, for the same period of time, recalled on the average only 14.2 ideas. Table I, also shows that the difference between the means is 44.32, σ 2.08 with a critical ratio of 21.30. Highly motivated reading is very superior to reading under distraction with no definite set.

Verbal reports. The reports of the *Os* of Group A show that most of them were under a prepotent determining tendency. They were 'set' to learn the ideas, as the following excerpts illustrate.

Ba: "I read the passage, trying to remember all the minor details, concentrating on concrete pictures."

Co: "I read the paragraph with the intention of making a very decided impression on my mind of the facts and details of the paragraph."

The reports of the *Os* from Group B show the influence of shifting 'sets.' Some of them read the material for a period, and then shifted to a listening 'set;' some merely read; others read and recalled the details; while others listened to the oral reading. Many, as the following excerpts show, fell into the first category.

My: "I tried to concentrate on the oral reading and on the reading of the printed page. I found that I could not concentrate on both at the same time."

Sw: "I had a very hard time trying to make my mind think of two things at once. By trying to think of two things at once it was almost impossible to really understand what I was reading and what the reader was saying."

Wo: "I tried to listen to both at the same time, and this failing, I devoted my attention to the written paper."

These *Ss* were not strongly determined to remember specific ideas which could have coalesced into organized patterns. As a consequence, the group recalled comparatively few of the ideas of the material.

The data indicate that prepotent determining tendencies may be aroused in the organism; and that they are the center about which the items of

[3] H. E. Garrett, *Great Experiments in Psychology*, 1926, 78.

experience integrate into a whole. Motivation is a prime prerequisite in a learning situation.

Nonsense Syllables

Experiment III. The purpose of Experiment III was to discover the influence of determining tendencies upon the immediate recall of nonsense syllables. Do sets of the organism influence the immediate recall of nonsense syllables? Are some sets more important than others in the acquisition of nonsense material?

Stimulus-material. Twelve nonsense syllables, every syllable composed of 3 letters, a vowel between two consonants, were glued on a cardboard, 24 x 24 in. The syllables were made from black gummed paper letters, an inch in height. On the cardboard the syllables were placed in a vertical position. The following are the syllables used: ZID, TOJ, RIK, DUS, ROS, CEK, MUR, LOM, CIH, ZUD, KEB, TOZ.

Procedure. The *Os*, 80 in number, were divided into 3 groups of approximately equal numbers. The instructions in Experiment I serve as a general pattern for the instructions in this experiment. Through instructions Group A was set to learn the nonsense syllables and tested for recall. These *Os* were designated the "motivated group." Group B was set to compose anagrams and tested for recall of nonsense syllables—the "anagram group." Group C was set to read the nonsense syllables and tested for recall—the "reading group." All three of the groups were given the same material for 192 sec.

Methods. Reproductions and verbal reports were secured from all the *Os*. Every correct reproduction was given a point count of one. Since there were but 12 syllables, the highest score possible was 12.

Results. Table I shows the means of the groups. Group A reproduced on the average 6.54 of the nonsense syllables; Group B, 4.26; and Group C, 3.89. The difference between the averages of Groups A and B is 2.28, σ 0.72 with a critical ratio of 3.16; between Groups A and C, 2.65, σ 0.75 with a critical ratio of 3.52.

Verbal reports. In Group A, the *Os* were 'set' to learn the nonsense syllables. Several of them arranged the syllables into groups of three or four, and memorized them in a rhythmical manner. There was repetition of the syllables, but the repetitions were secondary to the heightened determining tendency as the following excerpts illustrate.

Do: "I memorized the syllables, four at a time, and kept repeating them."
Fr: "The syllables grouped themselves and formed a sort of rhythm as Zid-Toj, etc."
An: "I memorized the sound of each of the nonsense syllables in the order that they were on the card. I memorized about three at a time in a sort of a rhythmical way."

On the other hand, the *Os* in Group B were primarily set to compose

anagrams. The set in these cases was not to learn the syllables for reproduction, but to compose meaningful words. Examples are as follows:

Ma: "I did not try to remember how they were in the beginning."
Be: "In the nonsense syllable test I was trying to discover the words that the syllables would make."
Sy: "I tried to put them into some word that I could remember; I did not try to remember the syllables."

The reports of the *O*s in Group C, the reading group, show that they did not have a 'set' to remember the nonsense syllables except a few who shifted 'sets.' The following are examples:

Da: "I read the list of nonsense syllables over as many times as I could during the allotted time. I did not attempt to remember any of them."
No: "I read the nonsense syllables from top to bottom, keeping track of the number of times that I read the list."
Ma: "I read the list of syllables, which were held before me, several times. I made no attempt to learn them."

Reading, it seems, must be motivated to insure efficient retention of the material read.

The data in this experiment prove that determining tendencies influence the learning of nonsense material, and that the 'sets' of the organism do condition the amount and speed of the learning of nonsense syllables.

Geometrical Forms and Colors

Experiment IV. Since learning depends upon perception, present and past, Experiment IV was devised to discover what effect a determining tendency of the organism has upon the immediate recall of items from a visual field. More specifically, if *O*s are 'set' through instructions to remember or learn the colors of geometrical designs, will they recall forms equally well? When they are 'set' through instructions to learn the geometrical forms with a field of form and color, will they recall the colors of the designs?

Stimulus-material. Ten geometrical forms, each a distinct color, were arranged on a large square cardboard, 24 x 24 in. The forms were derived from the Standard Revision of the Binet-Simon Tests.

Procedure. The *O*s were divided into 3 groups and each group was tested at a different time. The instructions were patterned after those in Experiment I. Group A was 'set' for forms and tested for colors; Group B was 'set' for colors and tested for forms; and Group C was 'set' for forms and colors and tested for both. The same material was presented to all groups for 10 sec. The papers for Group A were graded on the basis of the correct colors written in the right design, and every correct color was given a value of one. Members of Group B received a score of one for every design reproduced correctly. Two sets of scores were obtained for Group C. Each paper was given a score of one for every correct form and another

score of one for each color, if written in the proper position. All the *O*s gave verbal reports.

Results. Table I shows the means of the groups. When the *O*s in Group A were instructed to learn forms from a perceptual field of forms and colors, and were tested for colors, they reproduced on the average 2.80 colors. On the other hand, the *O*s in Group C_e when tested for colors recalled on the average 4.70 colors, despite the fact their task was nearly double that of Group A. That is, they had to note and retain both form and color. The *O*s in Group B reproduced on the average 4.42 forms out of the perceptual field of forms and colors. In contrast, those in Group C_f when tested for forms reproduced on the average 6.34 forms, although they again carried a double load. The difference between the averages of Groups A and C_e is 1.90, σ 0.45 with a critical ratio of 4.22; and between Groups B and C_f, 1.92, σ 0.35 with a critical ratio of 5.48.

Verbal reports. The introspective data afford additional insight into the influence of the determining tendencies in perceptual learning. The following excerpts are from Group A.

Ha: "I took no notice of the colors, whatever."
Ma: "I tried to remember the designs, but paid no attention to the colors."
Jo: "I tried to remember the figures because they seemed to attract me and to hold most of my attention. I did not try to remember the colors."

From these typical reports, we see that the *O*s were under the influence of a task 'set,' and that this 'set' influenced the visual field. The *S*s saw intently the items they were searching for; extraneous items, they did not notice with any degree of accuracy.

The following reports are from Group B.

Fe: "I glanced at each color, not paying any attention to shapes."
Mb: "I attempted to learn the position of the colors on the sheet, and paid no attention to the forms."
Ga: "I just looked at the colors and tried to remember them, but I did not even think of the geometrical forms."

The *O*s were under 'sets' to study colors; shapes were not a part of the perceptual pattern.

On the other hand, the *O*s in Group C organized both forms and colors into patterns, as the reports show.

Ho: "I glanced at the top row, going from left to right, and then to the bottom row. Each time I was trying to remember the shape and color of every object."
Bu: "Starting with the upper left hand figures, I attempted to impress the successive figures and colors in my memory."
Ji: "I tried to learn the forms and colors in their correct order."

These introspections illustrate how form and color were integrated into total perceptual patterns.

The objective results and the reports demonstrate that determining tendencies influence the perceptual responses of the organism, and condition learning to a significant degree.

Pictures and Colors

Experiment V. Experiment V was designed to determine the influence of determining tendencies upon the recall of objects in a complex perceptual pattern of colored pictures. More definitely, if individuals are 'set' through instructions to learn objects as such, do they also remember the colors? Conversely, if individuals are 'set' through instructions to learn colors from a perceptual field of colored pictures, do they recall equally well the objects? In addition, if they are 'set' to learn both the pictures and colors, do they integrate them into organized patterns?

Material. Nine separate colored pictures were pasted on a square cardboard, 24 x 24 in. Five of the pictures contained a multiplicity of objects in various colors. All together there were 25 meaningful colored objects and 16 colors in the field. This arrangement of colored pictures produced a heterogeneous perceptual field.

Procedure. The *S*s were divided into three groups of approximately equal numbers. The instructions in Experiment I serve as a general model for those used in this experiment. Through instructions Group A was set to learn pictures and tested for colors; Group B was set to learn colors and tested for retention of pictures; and Group C was set for pictures and colors and tested for both. The material was presented to all groups for a period of 20 sec.

The papers of Group A were given a score of one for every color listed in its proper place; of Group B, a score of one for every correct picture or object; and of Group C, for every correct picture and every color, if listed with the right picture. The *O*s in all three groups gave verbal reports.

Results. Table I shows the means of the groups. The *O*s in Group A reproduced on the average 3.90 colors; in contrast, those in Group C_c reproduced 7.06 colors. The *O*s in Group B reproduced on the average 5.03 pictures; on the other hand, those in Group C_c reproduced 6.90. The difference between the averages of Groups A and C_c is 3.16, σ 0.39 with a critical ratio of 8.10; and between Groups B and C_p, 1.86, σ 0.34 with a critical ratio of 5.47. The *O*s in Group C had a double task—they had to integrate both colors and pictures into patterns.

Verbal reports. The following introspections are samples from Group A.

Pa: "I tried to learn the pictures without close observation of the colors."
Ra: "I did not try to remember the colors."
Fa: "I tried to make a picture of the objects in my mind so that after the card was gone I could still see them in my mind. I did not particularly notice the colors."

These *O*s were actively selecting and organizing their experience in terms

of the task 'set.' Colors were not, to any extent, integrated into the perceptual pattern.

On the other hand, the *O*s in Group B who were set to study colors, the pictures, as such, formed no definite part of the total patterns. Examples are as follows:

Ha: "The colors were more outstanding."
Ka: "I attempted to remember the color of the objects, not paying much attention to the objects."
Ga: "I spent my time trying to figure out the most outstanding colors."

The *O*s in Group C organized both pictures and colors into definite patterns, as shown by these examples.

Bu: "I looked at each picture and tried to put it definitely in my mind; then I looked at the colors of each picture and put the two together."
Ra: "I first looked at the pictures to see what they were about, and then I examined in detail the objects and their colors."
Wa: "I paired the initial letter of the object with the initial letter of the color, picking objects I could distinguish most readily."

The data show that determining tendencies, produced through instructions, do influence the items recalled from a complex perceptual field. There is organization, but this organization is conditioned by the set of the organism.

Puzzle Pictures

Experiment VI. The purpose of Experiment VI was to discover the effect of determining tendencies upon the comprehension of an intricate perceptual field. Do instructions, which 'set' the organism, influence the speed with which the individual notices certain items in a complex pattern?

Material. The material consisted of 3 so-called puzzle pictures which were chosen from a series of 12, published by Stoelting and Company. The entire 12 pictures were used in a preliminary test in order to evaluate them in terms of the difficulty they offered to finding the concealed human face. Those found to be approximately equal in concealment were: (1) Indian Land, (2) Nelson, and (3) Graft.

Procedure. Every *O*, 50 in all, came into the laboratory at an appointed time and took the test. Every one did all 3 pictures under 3 sets of instructions; however, the pictures, under the second and third instructions, were rotated in order to allow for any differences in difficulty of the pictures themselves, and to equate any possible practice effect.

There was a time limit of 5 min. for every *O* under all three instructions; that is, every *O* was given that much time to find the hidden face in the puzzle picture. As soon as *O* discovered the concealed face, or if (at the end of the 5-min. period) he had failed, *E* took the picture and recorded the time as shown by a stop watch. The 'general instructions' were always given first to every *O* so that he would have no definite clue to the concealed faces in the other puzzle pictures.

Objective data and verbal reports were secured from all the *O*s. The introspections were obtained in order to have a check upon the *O*s' time records, and to determine whether they followed instructions. The reports, moreover, afforded data for detailed analysis of the determining tendencies aroused in the *O*s.

Instructions. The 'general instructions' were:

General: I am going to give you a picture and I want you to name the objects you see in it. When I hand it to you begin immediately to call out the objects.

At the end of this phase of the experiment, the 'Right Instructions' and the 'Wrong Instructions' were read in the alternate cases. These were:

Right: This time I am going to show you a puzzle picture which contains a hidden picture. I want you to find this concealed form as quickly as you can. I suggest that you look for a human head; if you find some other figure report it. Say 'here' as soon as you have found the hidden picture.

Wrong: This time I am going to show you a puzzle picture which contains a hidden picture. I want you to find this concealed form as quickly as you can. I suggest that you look for a dog; if you find some other picture report it. Say 'here' as soon as you have found the hidden picture.

At the close of the tests, the following supplementary instructions were read:

Write how you did what the instructions told you to do in each of the puzzle picture tests.

Results. The objective data showed distinctly the influence of determining tendencies on meaning derived from a complex perceptual pattern. The average time for the 50 *O*s under 'General Instructions' was 296 sec., and only one *O* was successful in finding the concealed face. The average time for the *O*s under 'Right Instructions' was 107 sec.; and under 'Wrong Instructions,' 219 sec. Under 'General Instructions,' 2% of the *O*s were successful in finding the hidden picture; under 'Right Instructions,' 82%; and under 'Wrong Instructions,' 60%.

Reports: (1) Under 'General Instructions.' The following verbal reports are samples of those given by the *O*s under 'General Instructions.'

Sa: "I observed all objects in the picture possible and named them."

La: "In the picture I saw first the scene as a whole; then the larger objects separately, and finally the more minute ones."

Po: "I looked at the picture getting a general idea of it; then I started naming the different objects."

An analysis of all the reports showed that all the *O*s, with one exception, followed instructions. They first took the picture as a meaningful total; then they began to break the total pattern up into smaller meaningful units. More specific details came still later. The *O*s were not searching for a concealed picture; hence they failed to solve the puzzle picture. The internal organization of the individuals had much to do with the meanings derived from the pictures. The instructions conditioned an internal organization which forced the external pattern to conform, somewhat, to it.

(2) Under 'Right Instructions.' Under the right instructions the *O*s were set to find the true concealed picture. The following are random samples of the verbal reports.

Ha: "In the picture test I looked for combinations of light and dark places for the form of a face to appear. When I turned the puzzle picture slightly the face came into view."

Na: "In the test I was instructed to find a hidden face in the picture. This I did by increasing the tenseness of my eyes."

Jo: "I looked at the picture until the human face began to take shape."

The reports, on the whole, showed an intense searching on the part of the *O*s for a clue to the obscure face. The *O*s had a definite 'set,' and the perceptual field was organized in harmony with it.

(3) Under 'Wrong Instructions.' The *O*s under the wrong instructions were directed to find a hidden dog which, in truth, did not exist in the puzzle pictures. The reports which follow are samples.

Ca: "I concentrated on finding a dog to such an extent that I did not notice any other hidden picture."

Ke: "I turned the puzzle picture in random ways, first this way and that. I looked for a dog, but my imagination got the best of me. I found one, at least what I thought was one."

Ra: "I tried every angle and position of the picture trying to find a light space in the shape of a dog."

The reports demonstrated that as long as the *S*s maintained a definite 'set' to find a hidden dog, they did not discover the concealed human face.

The data from this experiment show that determining tendencies influence the speed with which individuals comprehend a complex perceptual field in a particular way; and that determining tendencies condition the meaning derived from intricate perceptual patterns.

Discussion

The results, objective and introspective, in each of the six experiments indicate clearly that the determining tendencies of the organism play a dominant rôle in human learning. The results of the experiments do not substantiate the machine theory of learning, but lend themselves rather to the organismic and configurational theory of organization.

The old Law of Temporal Contiguity no longer adequately describes learning. Items of experience, presented together, may not be recalled together. On the other hand, items of experience not presented together may be recalled together. An organism's experiencing two stimuli at the same time does not guarantee a linkage of the stimuli. The organism must cooperate functionally in the enterprise of connection forming. There must be a set which organizes the events into a unified pattern. Only when the sensory and motor responses fit into a pattern do they tend to persist for future use or reference.

There is need for an organizing principle in learning. The concept of determining tendency is such a principle, and this concept is presented as a substitute for the old mechanical theory. The association of items of experience is subordinated to the principle of determining tendency. Learning,

therefore, may be defined as the refinement of those sensory-motor responses by which an individual extends his insight into a given situation through the agency of certain determining tendencies. Learning takes place in reference to goals, but the goals are symbolized by determining tendencies of the organism.

The results of Experiment I indicate that the laws of contiguity and repetition cannot describe or adequately explain human learning. The *O*s in Group A, who were motivated to learn the ideas in the "Marble Statue," excelled greatly in the number of ideas reproduced, those *O*s in Group B, who counted and recounted the words. The former group recalled on the average 51 ideas; the latter, only 7 ideas. In fact, the results from each of the experiments in this study tend to explode the notion that contiguity and repetition are sufficient within themselves to account for learning.

The laws of association may be restated in terms of determining tendencies so that the old difficulties evaporate. Whenever a number of more or less discrete perceptions or ideas enter and fit about a determining tendency, they coalesce into a configuration. The individual items are held together by becoming a part of the total pattern. The organism is not a passive recipient of external energies, but is active and selects events for integration. Contiguous events will serve to initiate integrative functions only when they are arranged about a determining tendency. Repetition aids in integrating events into functional patterns when the organism is properly set or determined. In many cases, when the organism is highly set, organization in learning comes without repetition. Whenever *A* and *B* are associated, they are not experienced as two different events, but as members of one organized whole articulated about a determining tendency.

Conclusions

The results, objective and introspective, from all the experiments in this study supply the basis for the following conclusions.

(1) Determining tendencies influence greatly the organization, assimilation, and recall of sense material.

(2) Sets of the organism influence the organization and recall of nonsense material.

(3) Determining tendencies influence to a marked degree the perceptual field and its organization.

(4) Human learning is purposive to the extent that purposes are 'sets' of the organism.

(5) In human learning purposes are in a great measure engendered by the directness and clearness of instructions given the individual to fulfill.

THE RELATIONSHIP BETWEEN FAMILIARITY OF WORDS AND THEIR MEMORY VALUE

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The experiments to be described in this paper were designed to determine the influence of the element of relative familiarity upon the memory value of words. More specifically the problem may be stated as follows: Does the probability of a word's being recalled vary in any consistent way with the familiarity of the word? Has the relative familiarity of words any influence upon the readiness with which they enter into connections with other words as measured by recall? An answer to this problem would have both practical and theoretical significance for activities involving diction and particularly for the higher mental processes which are intimately related to word association. Since the familiarity of words and the frequency of their appearance are directly correlated, the following experiments bear implications for the factor of frequency in learned behavior.

METHOD AND PROCEDURE

Thorndike's *The Teacher's Word Book* was used as the source of words, the relative familiarities of which had already been reliably determined.[1] This book is a list of 10,000 most important words, determined by actual count in the most widely read sources, and includes the rating of each word on the basis of how widely and how frequently it appears. For each experiment a range of importance and equally spaced levels within the range were chosen. The lists of words were then compiled by selecting, more or less at random, the required number of words of each degree (rating) of importance. This index of familiarity is thus based on frequency of appearance in widely read material. That it is a valid measure of familiarity is an assumption made by the experimenter. With the *Ss* used in the experiments, college undergraduates, this assumption seems fairly safe.

In the test experiments each individual list contained words of each level of familiarity under consideration. A list was read once to a large group, and immediately after the reading the individuals wrote down as many words as they could recall. The total number and the percentage (of the possible number) of each grade of familiarity recalled by the whole group from the series of lists were taken as the measures of the memory value of the various grades. This method is an application of the memory-span technique, the frequency of immediate recall being the index of memory value.

Ten words were used in every list in these experiments. A series of 10 words was chosen as the optimum length of list, because it was not one which would

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[1] E. L. Thorndike, *The Teacher's Word Book*, 1921. The reader is referred to the *Word Book* for an adequate account of its compilation.

limit recall to a narrow part of the list and still was beyond the memory-span of most individuals. It was of course necessary that the size of the lists be greater than the memory-span to allow the possible superiority of certain words over others to show itself.

The first step in each experiment performed was the careful compilation of the lists of words to be used. This was done in such a way as to leave all factors constant but the one of familiarity. Asymmetrical distributions of the numbers of syllables in the individual words were avoided with respect to both the separate lists and the words of any level of familiarity. Repetition of the same initial letter within the list or grade of familiarity was likewise avoided. Care was taken to eliminate cases of obvious meaningful connection between adjacent words in a list. Only nouns were used in these experiments.

In every experiment but one, 5 grades of familiarity were used. The differences between the successive levels were always equated in terms of the differences between the rating scores given in Thorndike's lists. Ten words representative of each level of familiarity were chosen and arranged into 5 lists. The serial positions of each grade of familiarity within the list were rotated, from list to list, in such a way as to counterbalance the obvious influence of primacy-finality. If we represent the 5 grades with Roman numerals, I to V, grade I occupied positions 1 and 6 in list 1, positions 2 and 7 in list 2, and so on to positions 5 and 10 in list 5. Grade II occupied positions 2 and 7 in list 1, positions 3 and 8 in list 2, and so on to positions 1 and 6 in list 5. The other grades were similarly rotated. Thus in arranging the lists, the 2 halves of each were treated separately and familiarity levels given corresponding positions in the 2 halves.

Six lists were read at a single sitting. The first list read in each experiment was always a practice list of 10 arbitrarily chosen words. It was found in the preliminary experimentation that the first list of a series was always the favored one in terms of the number of words recalled. The remaining 5 lists (test lists in the experiments) gave roughly equal amounts of recall and showed that no consistent decremental tendency of any size had set in by the sixth list.[2]

Directions. Before an experiment was begun, each member of the group was supplied with a number of slips of paper, 1 for each list, and the following directions were read by the experimenter.

You have 6 sheets of paper. Please keep them in a pile throughout this experiment. I am going to read you 6 lists of 10 words. I shall read these 1 at a time, and at the end of each I shall ask you to write down as many of the words in that list as you can remember. When I have finished reading a list, I shall say, 'Ready, now write,' and give you 20 sec. in which to write as many of the words as you can remember. When the 20 sec. are up, I shall say, 'Stop, raise your pencil.' Then, we shall proceed to the next list, and so on.

The sheets of paper are for you to write on. When I signal to stop writing, you are to place that sheet on the bottom of the pile and write on the top sheet the next time. Before reading a list, I shall tell you to write a number at the top of the sheet of paper on which you are prepared to write.

Do not write your name on any sheet. This is not a spelling test. Do not fail to write a word down because you are afraid you cannot spell it correctly. Write as plainly as possible, and at the same time as fast as you can. You may disregard the order in which the words are read. Do not touch pencil to paper while I am reading

[2] The separate experiments were arranged in such a way as to furnish a check on the influence of order of reading lists.

a list. Do not look at what your neighbor is writing. You will only lose time. Do not ask any questions after the experiment has begun.

The procedure described in these directions was followed in all experiments. Different groups of *Ss*, classes in introductory psychology, were used in each of the 9 experiments.[3] The words were read at a rate of 1 per sec. A period of 20 sec. was determined to be one barely giving the *Ss* time to finish writing their recalls. The time between the recall of one list and the reading of the next was 8 sec.

The records thus obtained furnished the particular words recalled from each list by every individual in the group. These raw data were tabulated on master sheets, and the total frequency of recall for each word in the 5 test lists determined. The total frequency of recall for each degree of familiarity was then computed by adding the frequencies of the 10 words representing the degree. The possible number or frequency of recalls for each level of familiarity was easily obtained by multiplying the number of individuals in the group times the number of words in the lists of the particular level of familiarity. From these the percentage of recall was determined.

The results obtained in the 5 test experiments are presented in Tables I and II. These tables show the total frequency of recall and the percentage of recall for each level of familiarity involved in an experiment.

The Test Experiments

Experiment 1. In this experiment 5 degrees of familiarity were used.

The range of familiarity was that of the 2500 most important words in Thorndike's list. The 5 degrees represent successive steps of 500 words.[4] Thus the words of the most familiar level were chosen from those ranging from 1 to 500 in importance, and the least familiar from those ranging from 2001 to 2500 in importance. The 6 lists, 1 practice and 5 test lists, were read to 2 groups of *Ss*. The same order of reading was used with both groups. There were 33 *Ss* in one group, 48 in the other. The data for the total 81 *Ss* were treated together. Since there were 10 words of each degree of familiarity in the test lists, the possible number of recalls for each degree was 810.

Tables I and II show under caption "1" the total frequency of recall in Experiment 1 for each of the 5 levels and the percentage each is of the possible number. The Roman-numeral stubs in these tables represent relative levels in order of decreasing familiarity. Thus, for the first level (most familiar) in Experiment 1, one may see that 478 words were recalled, which is 59% of the possible number.

These figures indicate that the most frequently recalled words were those from the most and from the least degrees of familiarity used. The frequency of the least familiar is slightly greater than that of the most familiar.

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[3] The experiments covered a period of three years. No class was used more than once in an experiment.

[4] See page 127 in the *Word Book*.

Experiment 2. Ten degrees of familiarity were used in Experiment 2. They were chosen by steps of 500 as in Experiment 1 and covered the range of the 5000 most important words. Thus this experiment differed

TABLE I

THE RECALL FREQUENCIES OF THE RELATIVE LEVELS OF FAMILIARITY INVOLVED IN EACH OF THE 5 TEST EXPERIMENTS

Relative levels of familiarity	Experiments				
	1	2	3	4	5
I	478	424	432	299	491
II	465	390	381	272	447
III	401	344	351	244	404
IV	361	361	424	246	463
V	489	335	453	346	512
VI		342			
VII		336			
VIII		304			
IX		367			
X		431			
No. S	81	73	90	82	101
Poss. Freq.	810	730	900	656	1010

TABLE II

THE PERCENTAGES OF THE POSSIBLE FREQUENCY RECALLED FROM THE RELATIVE LEVELS OF FAMILIARITY INVOLVED IN EACH OF THE 5 TEST EXPERIMENTS

Relative levels of familiarity	Experiments				
	1	2	3	4	5
	% σ_p	% σ_p	% σ_p	% σ_p	% σ_p
I	59±1.7	58±1.8	48±1.7	46±1.9	49±1.6
II	57±1.7	53±1.8	42±1.6	41±1.9	44±1.6
III	50±1.8	47±1.8	39±1.6	37±1.9	40±1.5
IV	45±1.7	49±1.9	47±1.7	38±1.9	46±1.6
V	60±1.7	46±1.8	50±1.7	53±1.9	51±1.6
VI		47±1.8			
VII		46±1.8			
VIII		42±1.8			
IX		50±1.9			
X		59±1.8			

from the last in the number of steps used and in the range of difference in familiarity of the words.

The first 5 levels were the same 5 used in Experiment 1, although an entirely new set of words was selected. Ten lists of words were compiled in order to give each degree every possible position by rotating the order from list to list. In this experiment a list contained only 1 word of each level; in all other experiments there were 2 to a list. A group of 73 *Ss* was used. The practice list and 5 test lists were read on one day; 3 days later, the remaining 5 lists were read. Each degree

of familiarity was represented by 10 words in the test lists, hence the possible frequency of recall for any one degree was 730.

The columns captioned "2" in Tables I and II show the frequency and the percentage of recalls for the 10 levels of familiarity. These figures indicate that recall favors the two extremes of familiarity over the intermediate degrees, and here again the least familiar words have a slight advantage over the most familiar. The 6 intermediate degrees of familiarity vary irregularly in frequency of recalls; they are all inferior, however, to the 2 degrees at either extreme.

The fifth levels (V) in Experiments 1 and 2 included words of the same absolute degree of familiarity. In the first experiment they were the

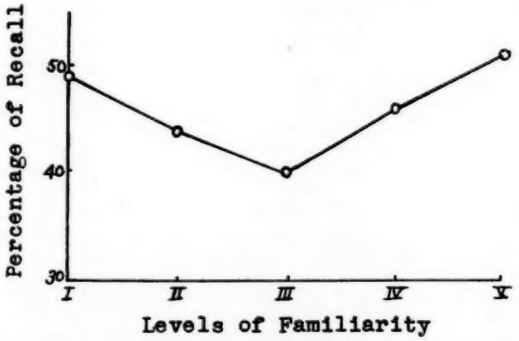

FIG. 1. CURVE FOR THE PERCENTAGES OF RECALL FOUND IN EXPERIMENT 5

least familiar words and were recalled most frequently; while in the second, where they were of intermediate degree, they were recalled relatively infrequently. It is also worth noticing that level IV of Experiment 1 and level VIII of Experiment 2, corresponding relative levels in the two experiments, received fewest recalls in both.

Experiment 3. Five widely separated grades of familiarity, covering the 10,000 most important words in Thorndike's lists, were chosen for this experiment.

In Thorndike's list the credit-numbers 3, 6, 9, 12, and 15 were taken as the levels of familiarity from least to most familiar.[8] Ten words for each of the 5 credit-numbers were selected from the *Word Book* and 5 test lists constructed on

[8] Refer to the *Teacher's Word Book* for the meaning and derivation of these credit-numbers.

the plan used in Experiment 1. These 5 lists and 1 initial practice list were read to a group of 90 individuals. The possible number or frequency of recalls for each level (credit-number) was thus 900.

Tables I and II present under "3" the frequency and percentage of recall for each of the 5 degrees of familiarity. The superiority of the most and the least over the intermediate grades of familiarity is here apparent, as well as the superiority of the least familiar over the most. The rank of the 5 levels of familiarity in terms of frequency of recall, from highest to lowest, is: V, I, IV, II, III.

Experiment 4. This experiment was a repetition of Experiment 3, omitting 1 of the test lists. Otherwise the conditions were exactly the same.

In the list omitted, the fourth (IV) level of familiarity held the most favorable serial position, the final position; the initial position, also a favored one in recall, was held by the least (V) familiar level.[6] The practice list and the 4 test lists were read to a group of 82 *Ss*. Since here there were only 8 words for each level of familiarity (2 to a list), the possible frequency for each was 656.

Tables I and II under caption "4" show the total frequency and the percentage of possible frequency for each level. The general trend of these figures is in agreement with the trend of those of the fourth columns for Experiment 3. When frequency of recall is plotted against familiarity, the same U-curve is evident. There are minor differences, however. The difference between the frequency of recall for the most and for the least familiar is relatively greater in this experiment than in the preceding one. The rank order of the 5 levels in terms of frequency is: V, I, II, IV, III.

These results are evidence of the stability of the chief relationship found in these experiments. The serial position of level IV in the list omitted in this experiment accounts for the only striking difference between the results of Experiments 3 and 4, the inversion of the frequency ranks of levels II and IV. The final word was recalled most frequently in almost every list in these experiments. The effect of omitting a list, and one in which level V had an advantageous position, was not great enough, however, to mask the consistent thread running through the results of all the preceding experiments: the more frequent recall of the extreme degrees of familiarity than of the intermediate degrees.

Experiment 5. The word lists compiled for Experiment 3 were used in Experiment 5.

[6] The sigmas of the percentages, presented in Tables II and III, were obtained from the tables published by H. A. Edgerton and D. G. Paterson, Table of standard errors and probable errors of percentages for varying numbers of cases, *J. Applied Psychol.*, 10, 1926, 378-391.

In this case, however, the lists were read in 2 different orders, neither of which was the sequence used in Experiment 3, to 2 groups of *Ss*. There were 47 *Ss* in one group, 54 in the other. The possible frequency of recall for a level was 1010 for the two groups.

Results. The last columns of Tables I and II present the combined results of both groups. The results of the 2 groups were treated separately and found to be directly comparable. The 5 levels of familiarity ranked the same in both groups in terms of frequency of recall. In the results of this experiment the 5 levels rank the same as they do in Experiment 3. The curve for the results of this experiment, in terms of percentage of recall plotted against degrees of familiarity, is presented in Fig. 1. The curves for Experiments 3 and 4 are almost identical with this one and those of Experiments 1 and 2 also show the general U-shape.

If we represent as a—b—c—d—e the order of reading the 5 lists in Experiment 3, we may represent the 2 orders used in this experiment as b—a—d—e—c and c—e—b—a—d. Since the same recall superiority of the extremes of familiarity are found in all 3 cases, it may be safely concluded that the order of reading the lists has no effect on the relationship.

The reliability of the results. Very apparent evidence of the reliability of the relationship found between familiarity and memory value is the consistency with which it occurs in the results of the 5 experiments. The results of all 5 show that the extremes of familiarity are more frequently recalled than the intermediate degrees; and they all show, furthermore, a slightly greater frequency of recall for the least than for the most familiar. Two further tests of reliability have been made.

The data (recall records) obtained in each experiment were divided into 2 parts and the frequencies calculated for each part separately. In Experiments 1 and 5 the data for the 2 groups of *Ss* used were treated separately; the data for the one group used in each of the other 3 experiments were arbitrarily divided into 2 halves. The relationship between recall frequency and familiarity value was found to be very similar for both halves of all 5 experiments. The rank orders for the two halves of Experiments 4 and 5 were identical. Both halves of all five show greater recall frequency for the extremes than for the intermediate degrees. Seven of the 10 halves (subgroups) show greater frequency of recall for the least than for the most familiar level.

The reliability of the differences between the percentages of recall for different levels of familiarity have been calculated. This was done for the differences between each pair of successive levels in all 5 experiments and also for the difference between the highest and lowest percentage in each.[7] The critical ratios for the differences between successive levels range from 0.4 (V and VI in Experiment 2) to 6.3 (IV and V in Experiment 1). The critical ratios for these differences in Experiment 5 (see Fig. 1) are: 2.2, 1.8, 2.7, 2.2. The critical ratios for the

[7] The average of all the critical ratios of the differences between successive levels in the test experiments is given below with the same for the control experiments.

differences between the highest and the lowest percentages in the 5 experiments are: 6.3, 6.7, 4.7, 6.0, 4.3.

The Control Experiments

In the experiments described above each test list contained words of all degrees of familiarity involved. Hence the context of each word was a series of words of different degrees of familiarity. A list was read only once, and the *Ss* recalled some of the words. There was thus an element of competition between the words of various familiarity levels for a place in recall. Here "competition" means only that the number of items presented is greater than the number recalled. The following experiments were performed in an attempt to control this element of competition between levels.

If all the words in a list are of the same degree of familiarity, and successive lists of varying degrees read, will the superiority of the lists of most and least familiar words be evident? If this is found to be the case, the competitive element is at least not totally responsible for the finding of the preceding experiments; but, on the other hand, the words of different levels of familiarity are of intrinsically different memory values. If negative results are found, then we are safe in concluding that *competition* within a list, or context, of intermediate degrees of familiarity is necessary for the superiority of the most and least to show itself.

A certain feature of the results of the test experiments already inclines one toward the second of these two possibilities. The consistency with which the U-curve occurs when frequency is plotted against familiarity regardless of the number or the absolute degrees of the levels of familiarity, is strong evidence favoring the efficacy of competition. The control experiments, however, furnish a definite answer to the question.

Experiment 6. A practice list and 5 test lists were read to 2 groups of *Ss*. Each list included words of only 1 level of familiarity. The 5 levels were the widely separated ones of Experiment 3; however, a different set of words representative of each level was selected. To one group, numbering 31 individuals, the test lists were read in order of decreasing familiarity, from the most to the least familiar level; the reverse order of reading was given the second group of 43 individuals. The percentage of possible frequency of recall for each level of familiarity found in the records of the total 74 *Ss* is presented in the second column of Table III. The possible number of recalls for each level was 740.

The results of this experiment do not indicate a regular change in memory value with changing familiarity. The data for the two groups, when treated separately, show the largest amount of recall for the medium level (III) and practically the same ranking for the other 4 levels.

Experiment 7. The 6 lists of Experiment 6 were read to 2 groups of *Ss* in different orders. If we symbolize each test list by the Roman numeral of the degree of familiarity of the words included, the order of reading to 19 *Ss* may be represented as: III, II, I, V, IV. The order of reading to 16 *Ss* was: IV, V, I, II, III. In the third column of Table III are the percentages of recall for the 5 levels obtained from the data of all 35 *Ss*. The possible number of recalls for a level was 350.

The results of Experiments 6 and 7 are very similar. In spite of the different order of reading the lists used with each of the 4 groups, the relative amount of recall for a list is almost the same in each group.

Experiment 8. The words of the test lists used in Experiments 3 and 5 were arranged into lists of equal familiarity. Thus the levels of familiarity in this experiment were the same as those of Experiments 6 and 7; the words were different. The test lists were read in the order of decreasing familiarity to a group of 119 *Ss*. The percentage of the possible frequency of recall for each level (1190) is presented in the fourth column of Table III.

With the exception of the least familiar level (V), the results show very little difference in amount of recall for the different levels. The decrease in percentage of recall with each successive level, from most to least familiar, looks like the cumulative effects of negative transfer.

Experiment 9. The word lists of Experiment 8 were read with the test lists in reverse order (from least to most familiar) to 2 groups of *Ss*. There were 80 individuals in one group, 76 in the other. The percentages of the possible frequency of recall (1560) found for the two groups combined are presented in the last column of Table III.

The results of both groups used in this experiment agree to the extent of showing level V to be first and level I to be second in amount of recall. Comparison of the fourth and fifth columns of Table III indicates fairly close agreement in percentages of recall for the first 4 levels—which were read in opposite orders in the two experiments. Level V, where they disagree radically, was the first test list read (the second list in the series) in Experiment 9, and was the last list to be read in Experiment 8.

The differences in percentage of recall obtained in the control experiments are smaller and less reliable than those found in Experiments 3, 4, and 5, in which the same five levels of familiarity were used. The critical ratios of the differences in percentage of recall between successive levels in the 4 control experiments range from 0.0 to 4.5. The range of critical ratios for differences in successive levels in Experiments 3, 4, and 5 is from 0.4 to 5.6.[8]

The largest differences in percentage of recall obtained in the control

[8] For the purpose of easy comparison the averages of critical ratios have been calculated. For Experiments 3, 4, and 5, this average with its sigma is 2.2 ± 1.2; and for the control experiments, 1.5 ± 1.3. This average for all the test experiments is 2.1 ± 1.4.

experiments are neither as large nor as reliable as those found in the test experiments. The critical ratios of the largest difference between the percentages of the 4 control experiments are: 4.4, 3.2, 4.0, 4.5.

Two different sets of words representative of the same 5 levels of familiarity were used in these control experiments. The results show that frequency of recall for the different lists is fairly consistent within the same set of words but varies with respect to the relative levels of familiarity. Apparently percentage of recall for a list is determined by some factor intrinsic to the list and a factor other than one of familiarity. This intrinsic

TABLE III

THE PERCENTAGES OF THE POSSIBLE FREQUENCY RECALLED FROM THE RELATIVE LEVELS OF FAMILIARITY INVOLVED IN EACH OF THE FOUR CONTROL EXPERIMENTS

Relative levels of familiarity	Experiments			
	6	7	8	9
	% σ_p	% σ_p	% σ_p	% σ_p
I	46±1.8	46±2.7	46±1.4	48±1.3
II	49±1.8	52±2.7	45±1.4	44±1.3
III	51±1.8	52±2.7	43±1.4	44±1.3
IV	40±1.8	45±2.7	43±1.4	42±1.2
V	42±1.8	40±2.6	38±1.4	50±1.3

factor is probably the degree of meaningful connection between the words of the list.

Changing the order of reading the lists has a very limited effect upon frequency of recall. The only case in which the order of reading shows any extensive influence on frequency is that of the list containing level V in the set of lists used in Experiments 8 and 9.

When each level of familiarity occupies one list (as in the control experiments) these 2 variables, intrinsic recall value of the list and order of reading, are left uncontrolled. In the test experiments, they were controlled by including each level of familiarity in each list of words. If the varyingly familiar words used in the control experiments are of intrinsically different memory values, the differences are very slight, and completely masked by other variables.

These control experiments show neither the superior recall of the extremes of familiarity nor the magnitude of differences in recall frequency for the various levels of familiarity found in the test experiments. Therefore it may be concluded that, for the particular degrees of familiarity involved in these experiments, *competition* with intermediate degrees is a necessary condition of the superiority of the extreme degrees.

On the number of words recalled in successive lists. The percentages of recall for successive lists of words were determined for each of the 9 experiments. With the exception of the first (practice) list, which received the greatest amount of recall, only small and irregular differences were found. In the test experiments, no difference in recall frequency was as large as the difference between the highest and lowest recalls for separate levels of familiarity. In all but 1 of the 9 experiments, the recall was greater for the first than the last test list, although in 4 experiments the recall of an intermediate list exceeded both. The average reduction in percentage of recall from the first to the last of the test lists was found to be 3.7%.[9]

TABLE IV

MEAN PERCENTAGE OF RECALL AT SUCCESSIVE LISTS IN ORDER OF READING

Successive lists	Mean % Recall	
	Test experiments	Control experiments
Practice	60	62
1	51	48
2	49	45
3	48	46
4	53	44
5	46	44

Table IV contains the mean percentages of recall for test and control experiments.[10] Each percentage in this table was obtained by averaging the percentages found in the separate experiments for the list of the particular position in the series. This table shows the magnitude and direction of change in recall frequency over the series of 5 lists. According to these figures, the change in recall frequency over a series of 5 lists, preceded by a practice list, is one of a gradual decrement.

In the test experiments the effect of this slight change in recall over successive lists was controlled in 2 ways. In the first place, the lists were arranged with each level of familiarity represented by words in each list. There were 2 words of each level of familiarity in every list except those of Experiment 2. In the second place, the order of reading the lists was changed with practically no effect upon the relative frequency of recall for the various levels. The same 5 lists, in Experiments 3 and 5, were read in three different orders with the same resulting positions of the levels of familiarity with respect to recall.

[9] This average is derived from the differences between the percentages of recall in the first and the last lists in the separate experiments.

[10] The last five lists read in Experiment 2 are not included.

On intra-serial position effects. Table V shows, for the test experiments and control experiments separately, the percentages of recall for words of each serial position. The percentages in the second column of this table were obtained by dividing the total number of words recalled at each position in the test lists of the test experiments by the number of words which could possibly be recalled. The percentages of the third column were determined in the same manner for the control experiments. The practice list is not included in these percentages.

According to these figures, the influence of finality is stronger and extends over more positions than that of primacy. Primacy-finality is probably the strongest factor operating in the determination of which words of a series will be recalled. Individual lists, however, show wide variations from the trend indicated in the table.

TABLE V

PERCENTAGE OF WORDS RECALLED AT EACH SERIAL POSITION

Serial positions	Test experiments	Control experiments
1	61	50
2	41	34
3	34	25
4	29	24
5	26	27
6	30	26
7	35	48
8	57	62
9	77	70
10	89	84

This primacy-finality influence was controlled in the test experiments by rotating the levels of familiarity in such a way that each level held every position in the series. It is still possible, however, that serial position effect might have conspired (by chance) with factors affecting the recall of the list (such as, order of reading and intrinsic memory value of the list) and thus produced the superiority of the extreme levels. For example, if the first list in the set received the greatest recall and level V occupied the final position in the list, level V would have an advantage over the others which occupied final positions in less favored lists. As a check on this possibility examination was made of the positions in all lists showing either extremely low or high amounts of recall. It was found that more often than not the extreme levels of familiarity were disfavored by this factor.

SUMMARY AND CONCLUSIONS

The results of 5 test experiments indicate that when a series of words, of varying degrees of familiarity, is read once to a group of *S*s, the imme-

diate recall records show greater frequency of recall for the words of either extreme of familiarity. Words of specified degrees of familiarity were chosen from Thorndike's *Word Book*. Underclassmen college students were used as *Ss*. Several lists of words were read in succession at one sitting. Rotating the positions of the degrees of familiarity within the lists, in order to give each degree every possible position, controlled the influences of variable recall in successive lists, serial position, and intrinsic memory value of a list. The influence of variable recall in a succession of lists was also controlled by changing the order of reading the lists. Different sets of words, levels of familiarity, and numbers of levels, were used in the experiments.

The fact that the superiority of the most and least over the intermediate degrees of familiarity occurred in recall regardless of the absolute degrees or the number of degrees used implies that the determining factor is a context of varyingly familiar words in which the extremes are favored, rather than the intrinsic memory values of the words themselves. This implication was tested in 4 control experiments in which each list contained words of only 1 degree of familiarity. The results of these experiments showed smaller differences in recall frequency for different levels than those found in the test experiments; and they showed no consistent superiority of any degrees of familiarity.

Any conclusion to be drawn from these results must be qualified by the conditions of the experiments. The index of familiarity used limits the conclusion to highly familiar words. It also limits the meaning of familiarity to sheer frequency of appearance. The variable of effect, for instance, which is generally accepted as the powerful factor in memory, may be ruled out in our conclusions.

It may be concluded that the familiarity of a word, relative to the familiarities of the other words with which it appears, is a factor influencing its memory value. In a series of words of varying degrees of familiarity, the most and the least familiar are recalled more frequently than the words of intermediate degrees. The determining factor, however, is a context of words of varying familiarity in which the extremes of familiarity are favored by memory, rather than the familiarity of the words themselves.

FEAR, ANGER, AND DISEASE: THEIR INTER-CORRELATIONS IN NORMAL AND ABNORMAL PEOPLE

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Physical diseases are said to intensify fear and anger reactions. When we are ill we are apt to be apprehensive and irritable. Stratton's studies suggest that physical disease somehow tends to leave a lasting impression on the organism so that even after recovery, the individual's resistance to anger reactions and possibly to fear reactions is permanently lowered.[1] More specifically, Stratton found that normal individuals with histories of several physical diseases tended to have more intense anger reactions and slightly more intense fear reactions than people without disease histories. In general, the relationship between number of diseases and fear was insignificant while the relationship between number of diseases and anger was positive and in several cases significant.

Since psychoneurotic and psychotic individuals are often fearful or irascible, it seemed worth while to repeat Stratton's studies, making use of both psychopathic and normal subjects. We hoped that we might also throw some light on the constitutional factor in mental diseases. In a previous study McNemar and Landis[2] had shown that the Willoughby E-M scale was not producing the same sort of record of emotivity which Stratton had obtained. For this reason we composed our own scaling methods and compared them directly with Stratton's.

EXPERIMENTAL PROCEDURE

In Stratton's work, fear and anger were studied by having every *S* keep a record of all fear and anger situations occurring over a week-end. These scores were then compared with the disease histories of the *S*s. For comparison with our method, we collected data on 103 college students using Stratton's tests and technique. For obvious reasons Stratton's week-end technique was not applicable to our hospitalized abnormal patients. We therefore constructed a list of 40 fear and 40 anger situations. These were developed into a questionary which permitted every

* Accepted for publication April 15, 1935.

[1] G. M. Stratton, Emotion and the incidence of disease, *J. Abn. & Soc. Psychol.*, 21, 1926, 19-23; Anger and fear: Their probable relation to each other, to intellectual work and to primogeniture, this JOURNAL, 39, 1927, 125-140; Emotion and the incidence of disease: The influence of the number of diseases and of the age at which they occur, *Psychol. Rev.*, 36, 1929, 242-253.

[2] O. W. McNemar and C. Landis, Childhood disease and emotional maturity in the psychopathic woman, *J. Abn. & Soc. Psychol.*, 30, 1935, 314-319.

S to indicate three degrees of fear or anger in each situation. As indicated in the accompanying questionaries, each situation could be answered in one of three ways: 'L' signifying little or no fear or anger; 'S,' some fear or anger; 'M,' much fear or anger.

Fear Questionary

Directions: The statements below refer to *fear* or *anxiety* situations. Before each statement are the letters L, S, and M [omitted below to save space]. Draw a circle around the letter which best expresses your usual degree of fear or nervousness with respect to that situation.

L means *Little* afraid or anxious, if at all.
S means *Somewhat* afraid or anxious.
M means *Much* afraid or anxious.

Normal			*Abnormal*			
L	S	M	L	S	M	
70	25	4	67	26	7	Riding fast in an automobile.
43	42	10	70	18	10	The boss calling me to his office.
36	28	37	39	19	41	Drowning.
29	33	36	39	22	39	Being held up at the point of a pistol.
40	36	24	49	28	21	Not being able to earn enough money to live on.
15	29	56	29	18	52	Death of loved ones.
42	45	10	60	24	15	Asking my boss for a favor.
43	36	21	55	24	21	The sight of a snake.
55	32	12	56	24	20	Going up in an airplane.
32	24	45	33	19	48	Catching some bad disease such as syphilis or cancer.
42	30	28	56	19	23	That some one may push me off when I am on a high place.
63	26	10	57	20	23	Committing some sin.
41	35	22	56	20	24	Robbers or thieves attacking me.
41	33	26	50	23	27	Being operated upon.
50	37	13	64	22	14	That other people will not like me.
49	32	19	50	25	25	A railway or subway wreck.
60	27	12	51	23	26	Handling a loaded gun.
50	36	14	60	25	14	A barking dog close at my heels.
62	29	9	72	18	11	Going into a dark cellar or basement alone.
38	40	22	59	23	18	Getting stage fright or being tongue-tied in talking with some important person.
64	21	14	59	19	21	Being struck by an automobile while crossing the street.
50	29	15	59	25	21	Losing my job.
59	23	18	48	24	27	Deep water.
42	42	15	51	29	20	Not being able to live up to what others expect of me.
44	28	28	52	17	30	I might cause some one severe injury or even death by my own carelessness.
66	32	2	58	32	10	Walking down an old and shaky stairway.
32	40	28	43	27	29	Being offensive to some one I like.
39	42	19	51	23	25	Getting an electric shock.
39	28	34	45	22	33	That I shall be physically disabled by some accident or disease.
64	16	20	57	18	24	Being struck by lightning in an electric storm.
34	32	34	43	22	33	Making a failure of my life.
55	34	11	64	24	13	Bees flying around me.
35	35	29	46	30	24	Falling from a high place.
64	27	10	61	21	18	Loneliness in my old age.

Normal			*Abnormal*			
L	S	M	L	S	M	
55	33	11	56	25	19	Someone driving into me when I am in a car or driving a car myself.
75	20	5	69	15	13	The opposite sex.
62	25	14	67	20	12	Going to a dentist.
64	25	11	61	19	20	Being hit by a stray bullet.
67	24	10	70	15	15	Large bugs.
58	26	17	55	21	25	That I have left something burning which might cause a fire.

ANGER QUESTIONARY

Directions: The statements below refer to *anger* or *annoyance* situations. Before each statement are the letters L, S, and M [omitted below to save space]. Draw a circle around the letter which expresses your usual degree of anger or annoyance with respect to that situation.

L means *Little* angered or annoyed, if at all.
S means *Somewhat* angered or annoyed.
M means *Much* angered or annoyed.

Normal			*Abnormal*			
L	S	M	L	S	M	
62	25	12	61	27	12	A slamming door awakening me at night.
40	40	19	54	28	18	To find a hair in the food that I am eating.
44	35	18	52	22	14	A person in an automobile I am driving telling me how to drive.
28	41	31	57	23	20	Being with some one who "puts on airs."
40	39	21	60	27	12	Just missing a street car.
45	44	10	55	31	14	To be disconnected while talking over the telephone.
32	40	28	34	32	34	Persons being inquisitive about my personal affairs.
30	39	30	57	25	17	Being told to mind my own business.
17	27	56	44	32	24	A person cheating in a friendly game.
23	50	27	40	30	29	To hit my finger with a hammer.
39	34	27	46	32	23	A person looking over my shoulder while I am writing.
20	40	41	24	38	38	To see dirty clothes lying around the room.
10	29	61	29	28	43	A person scolding me when I am doing the best I can.
34	32	34	47	21	24	My sweetheart dating another.
40	43	17	55	32	13	Finding a window stuck when I try to get it open.
19	48	29	42	29	24	Having the boss unjustly pile up work on me.
28	39	34	39	28	31	Someone telling me to hurry when I am already hurrying.
38	41	21	39	34	28	Oversleeping when I intended to get up early.
25	37	39	36	32	32	To see a person who is driving an automobile taking unnecessary chances.
16	46	39	40	35	24	To make some careless error in a game when I really knew better.
39	34	27	46	32	21	To hear a person sitting near me humming the tune during a musical number.
17	49	34	38	38	25	To have to wait for a person who is late for an engagement.
31	42	27	44	28	27	A stranger jostles or crowds me without apologizing.
19	30	50	32	28	37	An acquaintance pretending to be very good when I know he really is a scoundrel at heart.
22	44	32	46	28	23	Being forced to work overtime when I had planned to take the evening off.
31	50	20	48	31	21	A person asking me to repeat what I have said when he has not paid attention the first time.

Normal			*Abnormal*			
L	S	M	L	S	M	
16	42	42	39	35	26	To hear a mosquito buzzing around my face when I am trying to sleep.
25	48	27	42	32	25	Having to put up with some dumb or clumsy individual's work.
24	42	34	51	25	24	A companion refusing to tell me something after he has aroused my curiosity about it.
35	36	28	57	24	19	A person telling me to do something when I am just about to do it.
19	37	44	41	30	30	Being in the company of someone who acts superior to me no matter what I do or say.
37	42	21	42	35	21	Having a person persist in doing something in spite of my advice against it.
20	35	45	37	37	27	To hear a person reading the titles aloud during a moving picture performance.
18	48	34	32	34	34	To be hurt or hindered by another's carelessness.
30	34	36	45	25	30	Being laughed at.
29	40	31	49	29	22	A person saying "I told you so" when something I have done turns out wrong.
49	32	18	52	26	22	To hear cats fighting when I am trying to go to sleep.
42	52	7	71	19	9	To listen to political views which are not like mine.
58	31	11	59	27	13	Some one advising me when I have not asked for advice.
46	28	26	56	21	23	To have to kiss a relative when I do not want to.

In this questionary the fear situations were separated from the anger situations by a sheet listing 35 common diseases. Every *S* was requested to check all the diseases he had had.

Data were collected from 200 normal and 210 abnormal adult individuals. The normal group consisted of 100 male and 100 female college students. The abnormal *S*s were patients at either the Kings Park State Hospital or the New York State Psychiatric Institute and Hospital. Of the 210 patients, 110 were males and 100 females. As regards diagnosis 119 were schizophrenes, 45 manic-depressives and 46 psychoneurotics. No patient having an organic psychosis was included.

Comparison with Stratton's method. In order to compare our simpler method with Stratton's we gave Stratton's tests of fear and anger to 103 students in Brooklyn College who had taken our questionaries two weeks previously.[3] Of the 103 students, 35 failed to experience a single fear while 26 failed to experience a single anger over a week-end. These individuals were not considered in the comparison of the two methods. The *r* between Stratton fear scores and our fear scores was $+0.39 \pm .07$; and between Stratton anger scores and our anger scores was $+0.31 \pm .07$. It would seem from these *r*s and from the similar results obtained that our method is fairly comparable with Stratton's. Fear and anger scores obtained by a simple questionary are closely related to fear and anger scores obtained by the diary method.

[3] We are indebted to Professor G. M. Stratton for the numerical score values which he obtained on his diary form. We have used them to give numerical values to our reproductions of his diary form so that they might be correlated with our questionary.

Results

In Table I we have reported the relative distribution of 'L,' 'S,' and 'M' scores for our two groups. The chief differences to be noted are that as regards both fear and anger, the normal group report a greater percentage of 'S' (some fear or anger) judgments, while the abnormal group report a greater percentage of 'L' (little fear or anger) judgments.

TABLE I

Relative Distribution of Intensity Scores

	Fear			Anger		
	L	S	M	L	S	M
Normal	46%	33%	21%	31%	38%	31%
Abnormal	52%	24%	24%	44%	31%	25%

In Tables II and III we have compared our two groups as to intensity of fear and anger reactions. The scores reported in Table II represent a weighted summation of 'L,' 'S,' and 'M' judgments.

Since 'L' represents little or no fear or anger we assigned arbitrary values of (0) to 'L' judgments, values of (1) to 'S' (some) judgments and values of (2) to 'M' (much) judgments. According to the procedure,

TABLE II

Fear and Anger: Normals and Abnormals

	Fear Scores		Anger Scores	
	Av.	σ	Av.	σ
Normal	30.15	13.57	40.30	12.80
Abnormal	28.60	16.09	32.30	19.39

TABLE III

Reliability of Differences in Scores between Normals and Abnormals (Table II)

	D	$\sigma_{diff.}$	$D/\sigma_{diff.}$	Chances in 100
Fear	1.55	1.47	1.05	85
Anger	8.00	1.62	4.94	100

high scores indicate marked fear or anger while low scores indicate slight fear or anger.

Applying this information to our data, we note that the normal individuals tend to report that their fears and angers are more intense than those of abnormal individuals. With respect to total anger score our results are statistically conclusive. The chances are 100 out of 100 that the report of anger reactions of the normal individuals, as indicated by this question-

TABLE IV

COMPARISON OF NORMAL AND ABNORMAL INDIVIDUALS AS TO SPECIFIC DISEASES

Disease	Percentages		Diff.	Diff./$\sigma_{diff.}$	Sig. Diff.
	Normal	Abnorm.			
Appendicitis	4.6	2.2	2.4	1.4	
Asthma	3.0	1.8	1.2	.8	
Bright's disease	0.0	.4	− .4	0.0	
Persistent constipation	7.7	23.8	−16.1	4.7	Abnormal
Diphtheria	16.5	11.5	5.0	1.5	
Sleeping sickness	.4	0.0	.4	0.0	
Persistent headaches	14.2	14.1	.1	0.0	
Heart trouble	8.4	4.0	4.4	2.1	
Infantile paralysis	.4	1.8	− 1.4	1.4	
Infections:					
Discharging ear	18.5	13.2	5.3	1.6	
Abscessed tooth	19.3	12.8	6.5	1.9	
Other	6.9	2.6	4.3	2.2	
Influenza	31.2	12.3	18.9	5.3	Normal
Malaria	1.9	4.4	− 2.5	1.6	
Spinal meningitis	0.0	.4	− .4	0.0	
Nervous breakdown	3.8	11.9	− 8.1	3.1	Abnormal
Pleurisy	.4	1.8	− 1.4	1.4	
Pneumonia	12.7	8.8	3.9	1.4	
Rheumatism	5.7	7.5	− 1.8	.8	
Scarlet fever	16.5	16.8	− .3	.1	
Smallpox	4.2	2.6	1.6	.9	
St. Vitus' dance	2.3	5.7	− 3.4	1.9	
Tonsilitis	61.8	28.2	33.6	7.8	Normal
Tuberculosis	0.0	0.0	0.0	0.0	
Typhoid fever	3.1	2.2	.9	.6	
Measles	91.1	72.3	18.8	5.4	Normal
Mumps	52.1	32.6	19.5	4.3	Normal
Chicken pox	44.4	26.0	18.4	4.3	Normal
Whooping cough	52.5	37.9	14.6	3.2	Normal
Broken bones	12.7	7.1	5.6	2.1	
Poor digestion	10.0	19.4	− 9.4	2.9	Abnormal
Operations:					
Tonsils	50.2	29.5	20.7	4.6	Normal
Adenoids	35.9	16.8	19.1	4.9	Normal
Appendix	4.6	2.2	2.4	1.4	
Other	5.0	4.4	.6	.3	

ary, is more intense. As regards fear, the difference is less marked, the chances being only 85 out of 100.

Our finding of diminished emotional responses in abnormal individuals is in accord with the usual psychiatric observation. One of the outstanding symptoms in mental diseases (especially schizophrenia) is an emotional apathy. If we may be allowed to generalize from this questionary study our data indicate that this emotional apathy is selective. Fear reactions, which are associated with self-preservation, are but slightly diminished in intensity. Anger responses, which serve a social rather than a biological purpose, are sharply diminished.

Relation of fear and anger to incidence of disease. Of the thirty-five diseases listed in the questionary, the normal group reported an average of 6.1 and the abnormal group an average of 4.3 diseases (see Table V). Since the abnormal patients were older and were suffering from mental disorders, we attach no great significance to the greater number of diseases recalled by the normal individuals. It is interesting to note, however, that the differences in number of diseases recalled increase as we go back to earlier periods (Table V). The abnormal individuals recall an equal number of diseases for the adolescent period; 78% as many diseases as the normal individuals for the puberty period and only 57% as many diseases for the infantile period. This selective difference may be due to the fact that the

TABLE V

COMPARISON AS TO AVERAGE NUMBER OF DISEASES

Period of Occurrence	Normal	Abnormal	Difference
Birth to 6 yr.	2.8	1.6	1.2
6 to 14 yr.	2.3	1.8	.5
14 to 18 yr.	1.0	.9	.1
Total	6.1	4.3	1.8

abnormal *S*s were, on the whole, older and consequently, being more removed from their childhood, could not be expected to recall their childhood diseases as accurately as the younger normal *S*s.

On the basis of Table V, we should expect that the normal group would recall a reliably greater number of childhood diseases. As shown in Table IV, such is the case. The normal individuals reported a greater percentage ($D/\sigma_{diff.} = 3+$) of children's diseases such as measles, mumps, chicken pox, whooping cough, tonsilitis, adenoids and influenza. It is doubtful that these diseases actually occurred less frequently in the abnormal groups. It is more likely that the abnormal group found it more difficult to remember their childhood diseases.

Perhaps the most outstanding result of our disease comparison (Table IV) is the fact that more abnormal than normal individuals complain of digestive disorders. Persistent constipation and poor digestion are much more prevalent among abnormal than among normal individuals.

Our findings bearing on the relationship of number of diseases to fear and anger compare favorably with Stratton's. Although the relationship is by no means marked, there is a fairly consistent tendency for people who have had more diseases to report that they experience slightly more intense fear and anger (Table VI). Normal and abnormal individuals who have had 6 or more diseases tend to report more intense fears and angers than

normal and abnormal individuals who have had fewer than 6 diseases. Expressed in terms of statistical reliability (Table VII), the chances that individuals with 6 or more diseases will report more intense fears are 93 in

TABLE VI

RELATIONSHIP OF NUMBER OF DISEASES TO FEAR AND ANGER

No. diseases	No. Ss	Av. normal score		No. Ss	Av. abnormal score	
		Fear	Anger		Fear	Anger
0–1	3	34.00	31.00	41	25.78	23.41
2–3	34	29.79	41.29	55	25.62	27.98
4–5	59	28.25	36.67	50	28.80	34.00
6–7	58	31.16	41.14	30	32.00	34.37
8–9	18	27.94	40.72	13	34.38	41.31
10–11	17	31.53	38.53	4	28.25	43.50
12 and over	11	29.45	41.73	17	28.82	45.88
1–6	96	29.05	38.80	146	28.55	29.15
6 or more	104	30.90	41.05	64	31.55	39.00

100 for normal and 96 in 100 for abnormal individuals. The chances that individuals with 6 or more diseases will report more intense anger reactions are 97 in 100 for normal and 100 in 100 for abnormal individuals.

TABLE VII

COMPARISON OF INDIVIDUALS WITH FEWER THAN SIX DISEASES WITH INDIVIDUALS WITH SIX OR MORE DISEASES

Emotion	Group	Diff.	$\sigma_{diff.}$	$D/\sigma_{diff.}$	Chances in 100
Fear	Normal	1.85	1.27	1.46	93
	Abnormal	3.00	1.54	1.95	97
Anger	Normal	2.25	1.24	1.81	96
	Abnormal	9.85	1.77	5.56	100

The slight relationship between number of previous physical diseases and the reported intensity of emotional response is perhaps best summarized by the correlation coefficients reported in Table VIII. Although very low, they are all positive. For both groups the correlations between anger and

TABLE VIII

CORRELATION COEFFICIENTS BETWEEN NUMBER OF DISEASES AND FEAR AND ANGER SCORE

	Normals	Abnormals
Fear and Disease	$+0.01 \pm .05$	$+0.06 \pm .05$
Anger and Disease	$+0.10 \pm .05$	$+0.30 \pm .04$

disease are higher than the correlations between fear and disease. Stratton, who obtained similar results, suggests that the greater connection between anger and disease may have been due to the fact that his procedure did little more than break the surface of fear, while it penetrated a somewhat

deeper layer of anger.[4] Another explanation might be that in present-day civilization we are better protected from fear situations but very frequently exposed to anger situations. The fact that both Stratton's *S*s and our own reported higher anger than fear scores probably indicates that the difference is due to some more basic factor than test procedure.

Turning our attention again to Table VIII, we note that for both fear and anger the correlations with disease are lower for the normal group. It appears that in abnormal individuals, the occurrence of disease is more apt to intensify fear and anger responses.

Fear and anger. In contrast to the low correlations existing between disease and fear and disease and anger, the correlations between fear and anger were significantly high.

Normal: Fear and Anger, $r = +0.55 \pm .03$
Abnormal: Fear and Anger, $r = +0.58 \pm .03$

Stratton, who also found a positive correlation between fear and anger, suggests that these two emotions are largely of a common quality and function.[5]

Item analysis. In studies of this nature, we often find that detailed study of specific items composing the questionary (in this case, situations) is more revealing than average group scores. For this reason we have compared our normal with our abnormal group with respect to each reported fear and anger situation.

Our procedure was as follows. The total number of 'L,' 'S,' and 'M' judgments for each fear and anger situation was obtained for each group separately. These totals were then converted into percentages and the reliability of the difference in percentages between the two groups was determined. We considered a difference of percentages to be significant only when the difference divided by its sigma was equivalent to 3 or more. In general this item analysis confirms our average score findings (Table II). Abnormal individuals differ from normals in their anger reactions more markedly than they differ in their fear reactions.

Turning our attention to the fear situations, we may conclude from our item analysis that to show *some* fear in the following situations is more characteristic of normal individuals. To display *little or no* fear is more characteristic of abnormal *S*s.

(Items to which some fear is the normal response; little or no fear the response of the abnormal):

The boss calling me to his office.
Death of loved ones.
Asking my boss for a favor.
Robbers or thieves attacking me.
That other people will not like me.

In addition, normal *S*s display *some* fear in the following situations more frequently than abnormal *S*s.

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[4] *Op. cit., Psychol. Rev.,* 251.

[5] Stratton, The relation of emotions to sex, primogeniture and disease, this JOURNAL, 46, 1934, 590-595.

(Items to which some fear is reported by the normals; not by the abnormals):

Going into a dark cellar or basement alone.
Getting stage fright or being tongue-tied in talking to some important person.
Not being able to live up to what others expect of me.
Getting an electric shock.

To display *marked* fear in the following situations is more typical of abnormal *Ss*.

(Items to which much fear is reported by the abnormals; not by the normals):

Committing some sin.
Handling a loaded gun.
Walking down an old and shaky stairway.
The opposite sex.

One observes from the above data that the fears claimed by the abnormal *Ss* are very personal. Unlike the normals, they claim that they are unmoved by what may happen to others or what other people may think of them. They say they tend to be concerned only with possible harm to themselves. Interpersonal situations, such as getting stage-fright and asking the boss for a favor, do not frighten them, very likely for the simple reason that they rarely enter into such situations.

As regards anger the two groups have but little in common. To become somewhat or very angry in the following situations is more characteristic of normal than abnormal *Ss*. Conversely, to display little or no anger is more characteristic of abnormal *Ss*.

(Items to which some or much anger is the normal response; little or no anger the abnormal):

Being with someone who "puts on airs."
Being told to mind my own business.
A person cheating in a friendly game.
To hit my finger with a hammer.
A person scolding me when I am doing the best I can.
Having the boss unjustly pile up work on me.
To make some careless error in a game when I really knew better.
An acquaintance pretending to be very good when I know he really is a scoundrel at heart.
Being forced to work overtime when I had planned to take the evening off.
A person asking me to repeat what I have said when he has not paid attention the first time.
To hear a mosquito buzzing around my face when I am trying to sleep.
Having to put up with some dumb or clumsy individual's work.
A companion refusing to tell me something after he has aroused my curiosity about it.
Being in the company of someone who acts superior to me no matter what I do or say.
To hear a person reading the titles aloud during a moving-picture performance.
To be hurt or hindered by another's carelessness.
To listen to political views which are not like mine.

In addition more normal than abnormal *Ss* show some anger in the following situations.

(Items to which some anger is the normal response; little anger the abnormal):

A person in an automobile I am driving telling me how to drive.
To be disconnected while talking over the telephone.
A stranger jostling or crowding me without apologizing.

More abnormal than normal *Ss* show little or no anger in the following situations.

(Items to which little or no anger is the abnormal response; not the normal):

To find a hair in the food that I am eating.
Just missing a street car.
My sweetheart dating another.
Finding a window stuck when I try to get it open.
To have to wait for a person who is late for an engagement.
A person telling me to do something when I am just about to do it.
Being laughed at.
A person saying "I told you so" when something I have done turns out wrong.

We notice that 28 of the 40 reported anger situations differentiate normal from abnormal *Ss*. In all 28 situations, the differentiation is in the same direction. The abnormal *Ss* state that they are but little moved by situations that the normal *Ss*

TABLE IX

SEX DIFFERENCES AS TO FEAR AND ANGER SCORES

Group	Sex	No. Ss	Fear		Anger	
			Av.	σ	Av.	σ
Normal	Men	100	29.4	13.30	41.6	11.00
	Women	100	30.9	13.78	39.0	12.81
Abnormal	Men	110	24.7	14.73	29.4	18.95
	Women	100	32.8	16.46	35.4	19.39

say arouse their anger. We may in part explain this emotional difference by the fact that anger is primarily a social reaction. We usually become angry with other people. Since the abnormal *Ss* (especially schizophrenic patients who made up more than half our group) avoid intimate contacts with other people we should expect them to report less intense anger reactions than normal individuals.

TABLE X

RELIABILITY OF SEX DIFFERENCES

Emotion	Group	Diff.	$\sigma_{diff.}$	$D/\sigma_{diff.}$	Chances in 100
Fear	Normal	1.50	1.92	0.78	78
	Abnormal	8.10	2.16	3.75	100
Anger	Normal	2.60	1.69	1.54	94
	Abnormal	5.95	2.65	2.25	99

Sex differences. The prevalent belief that women are more emotional than men is in accord with our data (Tables IX and X). Both normal and abnormal women report more intense fears than men. In the case of the abnormal group the sex difference as to fear is very marked. Abnormal women also report more anger than abnormal men. The situation is reversed for the normal group. Normal men report a slightly higher anger score than normal women.

Stratton found that the mean reactions of normal women both in fear and anger

were more intense than those of men.[6] The difference was considerably more marked in the case of fear.

Our results confirm Stratton's findings as regards fear but fail to confirm his findings as regards anger. Other experimenters have also failed to confirm Stratton's findings with respect to anger. Meltzer, using the controlled diary method, found that on the average, men experienced 39% more anger situations per week than women.[7] It would therefore seem that normal women have more fears than normal men while normal men are more easily angered.

The greater overt emotional expression of the abnormal women is confirmed by psychiatric observation. In psychiatric hospitals the number of women in restraint far exceeds the number of men.

In comparing normal women with normal men as to specific fear and anger situations, we find that to display fear or anger in the following situations is more characteristic (D/σ diff. $= 3+$) of women.

Fears—more characteristic of normal women.
Large bugs.
Handling a loaded gun.
A railway or subway wreck.

Anger—more characteristic of normal women.
To see dirty clothes lying around the room.

The sex differences are more marked in the abnormal group. To display fear or anger in the following situations is more characteristic of abnormal women than abnormal men.

Fears—more characteristic of abnormal women.
Robbers or thieves attacking me.
Being operated upon.
A railway or subway wreck.
Deep water.
Going into a dark cellar or basement alone.
Walking down an old and shaky stairway.
The sight of a snake.
That someone may push me off when I am on a high place.
Handling a loaded gun.
Bees flying around me.
Someone driving into me when I am in a car or driving a car myself.
Going to a dentist.

Anger—more characteristic of abnormal women.
To find a hair in the food that I am eating.
To see dirty clothes lying around the room.
To hear a mosquito buzzing around my face when I am trying to sleep.

In both the normal and abnormal groups the men failed to report a single fear or anger situation reliably more frequently than the women in their respective groups.

SUMMARY

The interrelations of fear, anger, and disease have been investigated in a group of 200 normal individuals and 210 abnormal individuals, making

[6] *Ibid.*, 593.
[7] H. Meltzer, Student's adjustments in anger, *J. Soc. Psychol.*, 4, 1933, 285-309.

use of specially prepared questionaries. On the basis of our *Ss*' verbal reports, which although only relatively reliable, are among best available criteria of possible overt behavior, we offer the following tentative conclusions:

(1) There is no relationship between reports of fear and disease and only a slight positive association between reports of anger and disease for both normal and abnormal individuals.

(2) The relationship between number of diseases and the reported intensity of fear and anger responses is more marked in the case of abnormal individuals.

(3) Normal individuals report somewhat more intense fear and considerably more intense anger reactions than abnormal individuals.

(4) Reports of fear and anger are intimately related. The correlation coefficient between fear and anger is +0.55 for normal and +0.58 for abnormal individuals.

(5) Abnormal individuals seem to experience difficulty in recalling early childhood diseases.

(6) More abnormal than normal individuals complain of digestive disorders throughout life.

(7) For both groups, women tend to report more intense fears than men.

(8) Abnormal women report more emotion than abnormal men as regards both fear and anger.

(9) Our test procedure and findings agree favorably with Stratton's.

THE RELATIONSHIP BETWEEN THE CHARACTER OF THE TRANSFER AND RETROACTIVE INHIBITION

By M. E. BUNCH and M. M. WINSTON, Washington University

Transfer and retroactive inhibition appear to be intimately related phenomena, and it is highly probable that the conditions which determine the amount of transfer in learning situations are also important conditioning factors of retroaction. The relationship, however, between the amount and character of the transfer from one problem to a second and the retroactive inhibition that occurs under the same conditions in the retention of the first problem, has been incompletely worked out. Some studies report results which suggest that when the two learning problems involved are similar, the relationship between the influence of certain variables upon the amount of transfer and their effect upon retroaction is an inverse one. For example, as the degree of learning of the first problem is increased, the amount of positive transfer to the second problem increases[1] and the amount of retroactive inhibition decreases;[2] electric shock punishment for errors on the first problem increases the amount of positive transfer to the second similar problem[3] and decreases the amount of retroaction that occurs in the retention of the first;[4] the older the two associations of the same functional strength shows more positive transfer to the second similar problem,[5] and the older of the two shows less susceptibility to retroaction under the same conditions.[6] If, however, the learning problems themselves are changed, the results indicate that as the degree of similarity between the two problems involved is increased, the amount of transfer also increases,[7] while this increase in similarity is, within limits, followed by

* Accepted for publication March 9, 1935.

[1] R. T. Wiltbank, Transfer of training in white rats upon various series of mazes, *Behav. Monog.*, 4, 1919, 1-65; Y. H. Ho, Transfer and degree of integration, *J. Comp. Psychol.*, 8, 1928, 87-100.

[2] J. A. McGeoch, The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262.

[3] M. E. Bunch and F. D. McTeer, The influence of punishment during learning upon retroactive inhibition, *J. Exper. Psychol.*, 15, 1932, 473-495; C. F. Ni, The influence of punishment for errors during the learning of the first maze upon the mastery of the second maze, *J. Comp. Psychol.*, 18, 1934, 23-28.

[4] Bunch and McTeer, *op. cit.*

[5] S. H. Britt, The relationship between transfer of learning and age of previous associations, this JOURNAL, 46, 1934, 113-116.

[6] S. H. Britt and M. E. Bunch, Jost's Law and retroactive inhibition, this JOURNAL, 46, 1934, 299-308.

[7] L. W. Webb, Transfer of training and retroaction: a comparative study, *Psychol. Monog.*, 24, no. 3, 1917; A. G. Bills, *General Experimental Psychology*, 1934, 279-287.

poorer retention, *i.e.* greater retroaction.[8] Also, the suggestion is found in a few studies that the amount of retroaction varies to some extent with the character of the transfer, *i.e.* whether positive or negative, from the first to the second problem.[9] In instances of either marked positive or negative transfer the two learning problems are similar but the particular relationship they bear to each other is such that in the latter case the mastery of the first problem interferes with the mastery of the second task instead of facilitating it as in the former case. On the basis of a transfer theory of retroaction, it would be expected that greater retroaction would occur under the conditions of negative transfer from the first to the second problem than would occur if such transfer were positive in character. The experiment reported here was conducted to obtain more factual data concerning this problem.

This experiment is concerned with the relationship between the amount of retroactive inhibition and the character of the transfer, *i.e.* whether positive or negative, from the original problem to the interpolated problem. The problem studied may be stated in the form of a question: is the amount of retroaction partly a function of the character of the transfer from the first to the interpolated learning problem?

Conditions of the Experiment

The conditions of the experiment must be of such character that the amount of retroactive inhibition which results from learning an interpolated problem can be determined and compared under two different test situations which are as nearly alike as possible, with the exception that in one case there is positive transfer from the original to the interpolated learning, while in the other instance there is negative transfer from the first to the second problem. To insure positive and negative transfer, *i.e.* beneficial and detrimental effect respectively, in the mastery of the second task, the problems were constructed, as described below, in accordance with the results of such studies as those of Bair, Culler, Hunter, Pearce, Wylie and Bruce, in the field of transfer.[10]

Materials. Paired-associate material was used. Three lists of nonsense syllables were used as problems, 10 pairs of syllables in each list. These three lists, designated A, B, and C, are shown in Table I. List A represents the original problem; Lists B and C represent the interpolated problems.

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[8] W. S. Hunter, Learning: IV. Experimental studies of learning, *Handbook of General Experimental Psychology*, 1934, 535-536.

[9] L. W. Webb, *op. cit.;* Bunch and McTeer, *op. cit.;* Britt and Bunch, *op. cit.*

[10] J. H. Bair, The practice curve, *Psychol. Monog.*, 5, 1902 (no. 19), 1-70; A. J. Culler, Interference and adaptability, *Arch. Psychol.*, 3, 1912 (no. 24), 1-80; W. S. Hunter, Habit interference in the white rat and in human subjects, *J. Comp. Psychol.*, 2, 1922, 29-59; B. D. Pearce, A note on the interference of visual habits in the white rat, *J. Animal Behav.*, 7, 1917, 169-177; H. H. Wylie, An experimental study of transfer of response in the white rat, *Behav. Monog.*, 3, 1919 (no. 5), 1-66; R. W. Bruce, Conditions of transfer of training, *J. Exper. Psychol.*, 16, 1933, 343-361.

To obtain positive transfer from the original problem to a second problem, the latter was constructed in the following way with regard to the former: the response-syllables in List A were used as the response-syllables in List B, but were paired with different stimulus-syllables. For example, the first pair of syllables in List A is XYV-ZOJ, the first pair of syllables in List B is XEH-ZOJ.

TABLE I

PAIRED-ASSOCIATE LEARNING MATERIAL

List A	List B	List C
XYV-ZOJ	XEH-ZOJ	XYV-QUJ
CEF-GIW	JYQ-GIW	CEF-GAH
LAJ-VEC	ZIV-VEC	LAJ-VUK
QOC-DAX	QUJ-DAX	QOC-XEH
JID-ZUK	GEX-ZUK	JID-NYQ
XEG-CYV	XOJ-CYV	XEG-ZIV
XAT-ZEQ	VUK-ZEQ	XAT-GEX
ZIH-XYJ	QIW-XYJ	ZIH-QIW
MEQ-VAF	YEM-VAF	MEQ-YAZ
WOJ-YUZ	GAH-YUZ	WOJ-YEM

To obtain negative transfer from the original problem to a second problem, the latter (Problem C) differed from the first problem (Problem A) in that the response-syllables were changed while the stimulus-syllables were kept constant. For example, the first pair of syllables in List A is XYV-ZOJ, the first pair of syllables in List C is XYV-QUJ.

Subjects and groups. Undergraduate students in Psychology 1, 104 in number, served as *Ss*. These *Ss* were divided into 5 groups on the basis of the conditions of learning and retention under which the work was done. The program of the conditions of work followed by each group is shown in Table II.

TABLE II

PROGRAM OF THE CONDITIONS OF WORK FOR EACH GROUP

Groups	No. Ss	Original learning	Interpolated learning	Retention
A	21	Problem A		Problem A
AB	23	Problem A	Problem B	Problem A
B	20	Problem B		
AC	20	Problem A	Problem C	Problem A
C	20	Problem C		

In order to determine the amount and the character, *i.e.* whether positive or negative, of the transfer from Problem A to Problem B (the one designed to show positive transfer), it was necessary to have two groups of *Ss* learn Problem B. One group, referred to hereafter as Group B, learned only Problem B, and the second group, known as the Group AB, learned Problem B after having mastered Problem A. The learning records of Group B are used as a standard with which to compare the learning records of Group AB on the same problem; namely B. The difference between the groups in the efficiency with which Problem B was

mastered, indicates the amount and the character of the transfer from Problem A to B.

In order to determine the amount and the character of the transfer from Problem A to Problem C (the one designed to show negative transfer), it was necessary to have two groups of *Ss* learn Problem C. Group C learned only Problem C, and Group AC learned Problem C after having mastered Problem A. The difference between the groups in the efficiency with which Problem C was mastered, indicates the amount and character of the transfer from Problem A to Problem C.

Groups AC and AB were given a retention test on Problem A immediately after the mastery of the second problem, B in one case and C in the other. The interval separating the original mastery of Problem A from the retention test of the problem was one week. To determine the influence of the second problem upon the efficiency with which Problem A was retained it was necessary to have another group learn Problem A and take a retention test on Problem A after an interval of one week, without having learned an interpolated problem. The retention scores of this latter group, called Group A, serve as a standard with which to compare the efficiency of retention under conditions of interpolated learning.

Method of procedure: (a) *Learning.* The 10 pairs of syllables which constituted a problem were presented visually by means of a memory-drum. Each pair of syllables was exposed for 5 sec. After the 10 pairs had been thus presented once, a recall trial was given in which only the first syllable of each pair was presented, and *S* was asked to recall, in writing, the one paired with it in the practice trial. Then a second practice trial was given in which each pair was exposed as before, followed by a second recall trial. This alternation of practice and recall trials was continued until *S* was able to recall correctly the second of each pair of syllables when only the first of each pair was shown. The order of the paired syllables within the list was varied for each practice trial; the order within each recall trial was the same as the immediately preceding practice trial.

As indicated, the criterion of mastery was one correct recall trial; *i.e.* one recall trial in which the second member of each pair was correctly recalled when only the first syllable was presented. The number of errors made in the recall trials and the number of practice trials required to attain the citerion of mastery were used as measures of learning efficiency. Failure to recall the second syllable of each pair, or giving a wrong syllable, constituted an error.

(b) *Transfer and retention.* One week after Problem A was mastered the *Ss* returned to the laboratory for a retention test on this problem. The retention test was conducted under the same conditions as those which were present during learning; that is, with the same E (the junior author), in the same laboratory room, and as nearly as possible at the same time of day. The *Ss* in Group A relearned this problem after the interval of one week without having learned a second problem during the interval. The *Ss* in Groups AB and AC learned Problems B and C respectively just prior to this retention-test on Problem A. The procedure used in regard to the learning of Problems B and C was the same as that described above.

Two methods of measuring retention were used: (a) recall errors, that is, the number of errors made in a recall trial before any relearning trials were given; and (2) the number of practice trials required to relearn the syllables to the criterion of mastery used in original learning.

Results

(1) The influence of learning a list of paired-associates (Problem A) upon the ability to master a second such list (Problem B) when the second problem consists of the same response syllables as the first but involves a new set of stimulus syllables. The influence of learning Problem A upon the ability to learn B is obtained by a comparison of the relative efficiency manifested by Groups AB and B in the mastery of Problem B. The individuals in Group B learned Problem B only; the individuals in Group AB learned Problem B one week after having learned Problem A. Problem B is the same as Problem A with respect to the response syllables but consists of a different set of stimulus syllables. The learning data are presented in Table III.

In mastering Problem B to one perfect trial, Group B required 22.6 trials and made 129.05 errors, while Group AB required only 16.9 trials and made only 109.00 errors. Thus, as a result of the previous training on Problem A the individuals in Group AB required 25.22% fewer trials and made 15.53% fewer errors in mastering Problem B than did the individuals in Group B—the control group. The differences between the two groups have a rather high degree of reliability statistically, as indicated by the fact that the difference in terms of trials is 3.34 times its σ and 3.79 times its σ on the basis of errors (Table III).

There is marked positive transfer from one paired-associate nonsense syllable problem to a second when the second involves only the substitution of new stimulus-syllables for old ones in the arousal of the same response-syllables used in the first problem.

(2) The influence of learning a list of paired-associates (Problem A) upon the ability to master a second such list (Problem C) when the second consists of the same stimulus-syllables as the first but involves a new set of response-syllables. The stimulus-syllables in Problem A were used as stimulus-syllables in Problem C, but in the latter problem, each stimulus-syllable was paired with a new response-syllable; so that Problems A and C may be regarded as identical on the stimulus-side but different on the response-side. The *S*s in Group C learned Problem C only; the *S*s in Group AC learned Problem A one week prior to the mastery of Problem C. The difference between these two groups in the efficiency with which Problem C was mastered, indicates the influence of Problem A upon the learning of Problem C. The data are shown in Table III.

In mastering Problem C, Group AC required 19.35 trials and made 102.25 errors, as compared with the record of 15 trials and 77 errors of

TABLE III

AVERAGE NUMBER OF TRIALS AND ERRORS MADE BY THE DIFFERENT GROUPS OF Ss IN THE DIFFERENT PROBLEMS

Groups	Problem A				Problem B				Problem C			
	Trials		Errors		Trials		Errors		Trials		Errors	
	av.	σ	av.	σ	av.	σ	av.	σ	av.	σ	av.	σ
B					22.6	1.31	129.05	4.59				
AB	14.2	.5	71.2	1.72	16.9	1.07	109.00	3.67				
B–AB					5.7		20.05					
$\sigma_{diff.}$					1.70		5.28					
Diff./$\sigma_{diff.}$					3.34		3.79					
%AB<B					25.22		15.53					
C									15.0	0.89	77.0	2.29
AC	15.7	.52	74.2	1.16					19.35	1.31	102.25	2.46
AC–C									4.35		25.25	
$\sigma_{diff.}$									1.59		3.36	
Diff./$\sigma_{diff.}$									2.73		7.51	
%AC>C									29.0		32.79	
A	14.7	.69	73.0	1.78								

Group C. The influence of mastering Problem A upon the ability to learn Problem C, was detrimental. To master Problem C to the same criterion of learning, the individuals in the Group AC required 29.0% more of trials and made 32.79% more errors than did the *Ss* of Group C. There is, thus, marked negative transfer from the mastery of one paired associate problem to the learning of a second, when the latter requires the same stimulus-items used in the first problem to be associated with different response-syllables. As indicated by the ratio of each difference to its σ (Table III), the differences between the transfer group, Group AC, and the control group, Group C, are fairly reliable statistically: the chances are over 99/100 that if the work were repeated, negative transfer would occur under these conditions.

TABLE IV

DATA FOR RETENTION AND RETROACTIVE INHIBITION

Groups	No. *Ss*	Relearning				Recall		% saving	
		Trials		Errors		Errors		Trials	Errors
		av.	σ	av.	σ	av.	σ	av.	σ
A	21	4.00	.50	12.38	1.66	4.95	.21	73.99	83.52
AB	23	6.43	.64	20.00	1.10	6.56	.24	56.71	72.05
AC	20	8.80	.54	30.80	1.98	8.70	.24	46.36	58.46
AB−A		2.43		7.62		1.61		17.28	11.47
$\sigma_{diff.}$		0.82		1.97		.32			
Diff./$\sigma_{diff.}$		2.96		3.86		4.86			
%AB>A		60.75		61.55		32.52			
AC−A		48.00		18.42		3.75		27.63	25.06
$\sigma_{diff.}$		0.74		2.58		.32			
Diff./$\sigma_{diff.}$		6.44		7.10		11.43			
%AC>A		120.00		148.80		75.75			

(3) The amount of retroactive inhibition present when there is positive transfer from the original to the interpolated problem. It has been shown above that there was positive transfer from Problem A to B; that is, the learning of Problem A had a beneficial effect upon the ability to master Problem B. The question now concerns the influence of learning Problem B upon the efficiency with which the original problem, (A), was retained. This question involves a comparison of the retention scores of Group A, which relearned the problem after an interval of one week without having learned a second such problem during the interval, with the retention scores of the individuals in Group AB, who mastered Problem B just prior to the retention test of the original problem. The retention records on Problem A of the two Groups AB and A may be compared on the basis of (1) errors in recall, (2) the number of trials and errors made in relearning the original

problem to the criterion of mastery, and (3) saving scores; *i.e.* the percentage saved over the interval as indicated by the difference between relearning and original learning scores. Recall, relearning, and saving scores are presented in Table IV.

Problem A was retained for an interval of one week much more effectively under ordinary conditions of retention than under conditions of interpolated learning. For example, there was an increase of 32.52% in the number of errors made in the recall test by the individuals in Group AB as compared with the scores of the *S*s in Group A. In terms of relearning scores of Problem A there was an increase of 60.75% in the number of trials required, and an increase of 61.55% in the number of errors made by Group AB as compared with Group A. When no second problem was interpolated during the interval, the amount retained for the period of one week was 73.99% in terms of trials and 83.52% in terms of errors. The corresponding retention values for the group which learned the second problem, during the interval, are 56.71% in terms of trials and 72.05% for errors. There was a greater percentage of saving for the interval in terms of both trials and errors when no second problem was mastered during the interim, than under conditions of interpolation. These differences have a rather high degree of reliability, as indicated by the ratio of each difference to its σ, shown in Table IV. These data indicate, then, that under conditions of positive transfer between original and interpolated learning, there is marked retroactive inhibition in the retention of the first learned problem as the result of the acquisition of the second.

(4) The amount of retroactive inhibition present when there is negative transfer from the original to the interpolated problem. It has been shown in an earlier section that the mastery of Problem A has a detrimental effect upon the learning of Problem C. The amount of retroactive inhibition that occurs under the conditions of negative transfer from the original to the interpolated problem, is obtained from a comparison of Groups AC and A on the basis of the efficiency with which Problem A was retained for the interval of one week. The retention data are shown in Table IV.

The interpolation of Problem C between the learning and retention test of Problem A increased the number of trials required in relearning 120%; the number of errors made in relearning, 148.8% and the number of errors made in recall 75.75% over the corresponding retention scores of Group A which mastered no second problem during the interval. Under conditions of interpolated learning, there was a 46.36% saving in terms of trials and

58.46% saving for errors in the retention of Problem A as compared with 73.99% for trials and 83.52% saving for errors in the retention of the problem under normal conditions for an interval of one week. Statistical measures of reliability indicate that the differences between the two groups in the retention of Problem A, in terms of both relearning and recall scores, are highly reliable, as indicated by the fact that each difference is more than 6 times its σ (Table IV). Thus there is a marked detrimental effect of Problem C upon the retention of Problem A when the two problems are so related that there is negative transfer from the first learned problem (A) to the interpolated problem (C).

(5) The relative amount of retroactive inhibition under conditions of positive and negative transfer. In (3) and (4) above, it was found that marked retroactive inhibition occurred under conditions of positive transfer and also under the condition of negative transfer from the original to the interpolated problem. The question, as to whether the amount of retroaction is partly a function of the character of the transfer from the first to the interpolated learning problem, can now be considered. This question involves a comparison of the differences between Groups AB and A with the differences between Groups AC and A on the basis of the retention scores. The increase in the relearning scores, as a result of the acquisition of the interpolated problem, was 60.75% for trials and 61.55% for errors under conditions of positive transfer, while the corresponding retroaction from interpolation under conditions of negative transfer was 120.0% for trials and 148.8% for errors. A comparison on the basis of recall scores shows a similar result; namely, greater retroactive inhibition under conditions of negative transfer; *e.g.* there was an increase of 32.52% in the number of errors in the recall test under conditions of positive transfer, as compared with an increase of 75.75% when there was negative transfer from the first to the second problem.

The difference between the efficiency of retention of Problem A under the two transfer conditions in terms of saving scores also indicates greater retention, *i.e.* less retroactive inhibition, when the first problem facilitates rather than inhibits the mastery of the second problem. Under the positive transfer condition there was a difference in the percentage saved over the interval of 17.28 for trials and 11.4 for errors with respect to Group A. The corresponding differences under conditions of negative transfer with the same control group (A) were 27.63 for trials and 25.06 for errors. Thus all the measures of retention are consistent in indicating that greater

retroactive inhibition occurs under conditions of interpolated learning when the original problem inhibits rather than facilitates the mastery of the interpolated problem.

Before concluding that the amount of retroaction is a function of the character of the transfer from the original to the interpolated problem, the factors of (a) the amount of interpolated learning activity and (b) the amount of the transfer that occurs, whether positive or negative, merit comment, since both are variables in the present study.

Presumably the retroactive inhibition that occurred in the retention of Problem A under the positive transfer condition was a result of the acquisition of Problem B during the interval, while that which occurred under the negative transfer condition was a result of the mastery of Problem C during the interval. Problem B, the interpolated task under the positive transfer condition, was mastered in 16.9 trials with 109.0 errors, while Problem C, the interpolated task under the negative transfer condition, was learned in 19.35 trials with 102.25 errors (Table III). If these scores may be taken as a rough index of the amount of interpolated learning activity, which was designed to produce retroaction, that occurred under the two conditions, then the markedly greater amount of retroaction present under the negative transfer condition can scarcely be interpreted as a function of the small and irregular differences in the amount of interpolated learning activity.

The transfer from Problem A to Problem C was not only negative in character but was also greater in amount than the positive transfer that occurred from Problem A to Problem B. The positive transfer effect was 25.22% in terms of trials and 15.53% in terms of errors; the negative transfer effect was 29.0% in trials and 32.79% in terms of errors. Although the relationship between the degree of transfer and the amount of retroaction has not been worked out, it seems unlikely that the great difference in the amount of retroaction under the negative as compared with the positive transfer condition, is a function solely of the differences in the amount of transfer in the two situations. Especially is this unlikely since the difference between the two transfer conditions in the amount of transfer is slight in terms of trials, while the difference in the amount of retroaction under the two transfer conditions is pronounced in terms of both trial and error scores. Hence, it is concluded that the amount of retroaction is to some extent a function of the character of the transfer, *i.e.* whether positive or negative, to the interpolated task, and that the amount of retroaction is greater in the case of negative than in the case of positive transfer.

The results justify the following conclusions:

(1) The amount of retroactive inhibition resulting from the acquisition of an interpolated problem is to some extent a function of the character of the transfer, *i.e.* whether positive or negative, from the original to the interpolated problem.

(2) There is a greater amount of retroactive inhibition in the retention of the original problem, as a consequence of learning an interpolated problem, when the original problem transfers negatively rather than positively to ~~the~~ mastery of the interpolated problem.

PREDICTION AND CONTROL OF JUDGMENTS FROM TACTUAL SINGLE-POINT STIMULATION

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What, if any, use should be made of single-point stimulation and the judgments of *two* reported therefrom in the determination of the two-point threshold? In spite of the methodological and theoretical importance of this question no uniform practice or rules can be found in the psychophysical literature regarding *Vexirfehler*.[1] Some writers hold that single-point stimulation induces care on the part of the *O* and makes for better psychometric results,[2] while others believe that it induces a stimulus-attitude with the attendant stimulus-error. But even the writers who employ the single point in their experiments do not include the *two*-judgments in their calculation of the limen.[3] Data have often been discarded for certain sittings because of too many *Vexirfehler*, although no objective criterion of what constitutes a normal number of *two*-judgments is given.[4]

It is our purpose to show that we can predict the number of *Vexirfehler*, or *two*-judgments, from single stimulation, with almost the same certainty as we can predict the so-called 'normal' judgments from stimuli ordinarily used in determining the two-point threshold. We shall suggest definite, quantitative criteria whereby the number of *Vexirfehler* (hereafter referred to as V) may be judged to be normal or excessive, thus laying an objective basis for the acceptance or rejection of experimental data. In short, we hope to show that V can be adequately handled by the general psychometric function obviating the necessity for special theories to explain their occurrence.

Experimental procedure. In order to investigate the relations between the limen (L), the measure of precision (h), and the percentage of two-judgments from single stimulation (V), it is necessary to choose widely different regions of the

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[1] The junior author is responsible for most of the experimentation and calculations, the senior author for the report in its present form.

[2] G. H. Thomson, A comparison of psychophysical methods, *Brit. J. Psychol.*, 5, 1912, 205, speaks of the advantage of 'catch errors' in inducing care on the part of the *O*.

[3] Thus Thomson who used 'catch errors' in his experiments makes it clear that he did not include V in the calculations. G. H. Thomson, On changes in the spatial threshold during a sitting, *ibid.*, 6, 1914, 436.

[4] Thomson, in his article first referred to, states that more than 5 out of a possible 50 'catch errors' was the criterion of rejection of the results in a sitting. But why choose 10% as too many? As will be shown later, 10 and even 20% two-judgments from 0-separation may be normal, depending upon the values of h and L.

body having different sensitivities. Four regions, including 9 areas, were therefore selected as follows: three areas on the forehead, three areas on the chest, and one area each on the index finger, arm, and back. The ventral surface of the first phalange of the index finger was used with all *O*s. The dorsal surface, well above the elbow, was used with 3 *O*s, and an area 30 mm. below the elbow with a fourth *O*. One *O* only was used in investigating the back at the level of the 15th vertebra, 10 mm. from the bony portion of the spinal processes. Three areas were investigated on the forehead and the chest of 5 *O*s as follows: on the forehead, an area approximately 7 mm. above the eyebrows, including the midline of the head; the second area on the forehead began outside the distance needed for the widest separation in the first area, and the third area was likewise chosen so as to lie outside the area needed for determining the limen in the second area. The areas on the chest were chosen in a similar manner at the level of the first rib. The three areas will be referred to as Forehead 1, 2, or 3, and Chest 1, 2, or 3.

The limen in each area was first determined by the method of limits to insure a proper choice of stimuli to be used with the method of constant stimuli and to practice the *O*s. Seven stimuli were used in each area except on the index finger, where the threshold was too low to permit more than 3 or 4 separations with the stimuli at our disposal. In each case, except for the arm and back, it was possible to use single-point stimulus as a regular member of the series and still obey the rule that stimulus values be symmetrically spaced about the middle one. On the arm and back the results were as good as on the other regions, even though the step from zero to the next stimulus was greater than the steps between the other separations. In addition to the practice afforded by the preliminary observations, 25 series by the constant method were used before the regular series was begun.

The esthesiometers consisted of one set made at Bryn Mawr, ranging from 0 to 26 mm. in 1-mm. steps and weighing 9 grams, for use on the finger, forehead, and chest,[5] and a second set, the Stoelting aesthesiometers, ranging from 0 to 70 mm. in 5-mm. steps and weighing 35 grams, for use on the arm and back. Mention need not be made of the precautions adopted to insure uniformity of stimulation in all cases.

Five *O*s, including the writers, took part in the experiments. The other three *O*s were Dr. William D. Turner, instructor in psychology, and the Misses E. V. Fehrer and Lucena Quantius, graduate students in psychology. All were experienced in psychological observation. With the exception of the authors, the *O*s were unfamiliar with the purpose of the experiments beyond knowing that two-point thresholds were being determined.

Discussion of results. The values for the limen (L), the measure of precision (h), and the actual and calculated V are given in Table I. It will be remembered that in order to find the theoretical value for any stimulus-separation on the $\varphi(\gamma)$-hypothesis, one must find a value, γ, and from this get the desired percentage in Fechner's fundamental table.[6] γ is equal to

[5] Five grams were added when working on the chest, making a total weight of 14 grams.

[6] For the determination of h, L and theoretical percentages by Urban's method the reader is referred to E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280-293.

hL when the stimulus separation is zero, since hδ (δ is the difference between L and the stimulus-separation in question) and hL are identical in this special case.

A glance at Table I shows that the agreement between actual and calculated values of the number of two-judgments from 0-separation is remarkably good for

TABLE I

SHOWING THE VALUES FOR L AND h, THE ACTUAL AND CALCULATED PERCENTAGES FOR 0-SEPARATION AND THEIR DIFFERENCES (d)

	Index finger					Arm and back				
			% for o-sep.					% for o-sep.		
O	L	h	Actual	Calc.	d	L	h	Actual	Calc.	d
F	.865	.625	.23	.22	.01	32.86	.043	.19	.02	.17
H	1.27	.601	.15	.14	.01	33.95	.042	.15	.02	.13
K	1.20	.874	.07	.07	.00	35.98	.057	.09	.00	.09
♀	1.53	.881	.04	.03	.01	40.92	.067	.00	.00	.00
T	1.42	1.200	.01	.00	.01	45.67	.060	.00	.00	.00

	Forehead					Chest				
				% for o-sep.					% for o-sep.	
O	L	h	Actual	Calc.	d	L	h	Actual	Calc.	d
F (1)	1.70	.196	.30	.32	.02	(1) 6.40	.069	.27	.27	.00
(2)	1.55	.118	.35	.40	.05	(2) 9.61	.053	.25	.24	.01
(3)	3.03	.104	.25	.33	.08	(3) 7.93	.048	.30	.29	.01
H (1)	3.02	.128	.29	.29	.00	(1) 8.14	.063	.22	.23	.01
(2)	4.74	.126	.25	.20	.05	(2) 9.56	.046	.33	.27	.06
(3)	5.23	.102	.23	.22	.01	(3) 10.60	.037	.31	.29	.02
K (1)	3.23	.248	.17	.12	.05	(1) 11.67	.075	.10	.11	.01
(2)	4.89	.164	.17	.13	.04	(2) 16.39	.047	.16	.14	.02
(3)	5.00	.163	.15	.12	.03	(3) 16.14	.050	.14	.13	.01
♀ (1)	2.69	.176	.20	.25	.05	(1) 9.47	.065	.20	.19	.01
(2)	3.68	.152	.27	.21	.06	(2) 12.60	.045	.24	.21	.03
(3)	3.11	.226	.16	.16	.00	(3) 13.30	.036	.26	.25	.01
T (1)	2.75	.212	.35	.20	.15	(1) 8.44	.063	.25	.23	.02
(2)	3.16	.184	.20	.21	.01	(2) 7.83	.061	.27	.25	.02
(3)	3.64	.197	.20	.15	.05	(3) 16.04	.043	.18	.16	.02

a stimulus which is generally considered unfit for use as a regular member of the stimulus-series and for judgments which are usually regarded as anomalous.[7] Only 5 out of the 40 values can be regarded as failing to conform to the φ(γ)-hypothesis, but these 5 values (*F*, *H*, and *K* on the arm, *F* and *T* on the forehead) are no worse than the 5 poorest fits of the stimuli nearest the limen (Table II). Since Urban's method is especially designed to fit the stimuli nearest the limen best,[8] the

[7] Kincaid proposed that a special mathematical function be introduced to handle excessive V. M. Kincaid, An analysis of the psychometric function for the two-point limen with respect to the paradoxical error, this JOURNAL, 29, 1918, 227-232.

[8] The φ(γ)-hypothesis as weighted by Urban has been criticized by Thomson for not making any special 'tail' assumptions to fit extreme values. See G. H. Thomson,

goodness of fit of the 0-separation is all the more remarkable. In several cases the agreement between actual and calculated values of V is almost perfect, *e.g.* on the index finger for all *O*s.

If we compare the sums of the squared deviations between actual and observed values for V and the stimuli nearest the limen, we find no significant differences: the sum of the squared deviations for V is 0.1096, for the stimulus immediately below the limen 0.1076, and for the stimulus immediately above the limen 0.0754.

TABLE II

SHOWING THE ACTUAL AND CALCULATED PERCENTAGES AND THE DIFFERENCES (d) BETWEEN THEM FOR THE STIMULI IMMEDIATELY BELOW AND ABOVE THE LIMEN

	Index finger						Arm and back					
	R below L			R above L			R below L			R above L		
O	Actual	Calc.	d	Actual	Calc.	d	Actual	Calc.	d	Actual	Calc.	d
F	.23	.22	.01	.53	.55	.02	.44	.43	.01	.61	.55	.06
H	.42	.41	.01	.68	.73	.05	.44	.41	.03	.58	.52	.06
K	.40	.40	.00	.84	.84	.00	.51	.47	.04	.60	.63	.03
Q	.22	.25	.03	.74	.72	.02	.41	.46	.05	.69	.65	.04
T	.23	.24	.01	.84	.84	.00	.42	.48	.06	.65	.64	.01

	Forehead						Chest					
	R below L			R above L			R below L			R above L		
O	Actual	Calc.	d	Actual	Calc.	d	Actual	Calc.	d	Actual	Calc.	d
F (1)	.43	.46	.03	.55	.53	.02	(1) .48	.48	.00	.58	.60	.02
(2)	.44	.46	.02	.60	.53	.07	(2) .40	.48	.08	.57	.57	.00
(3)	.56	.50	.06	.54	.55	.01	(3) .48	.44	.04	.56	.53	.03
H (1)	.52	.50	.02	.60	.57	.03	(1) .56	.49	.07	.62	.63	.01
(2)	.47	.45	.02	.50	.52	.02	(2) .40	.46	.06	.47	.56	.09
(3)	.44	.49	.05	.59	.54	.05	(3) .52	.45	.07	.50	.53	.03
K (1)	.30	.45	.15	.65	.59	.06	(1) .41	.35	.06	.36	.51	.15
(2)	.39	.42	.03	.50	.51	.01	(2) .50	.49	.01	.54	.59	.05
(3)	.48	.50	.02	.60	.59	.01	(3) .39	.50	.11	.62	.61	.01
Q (1)	.44	.43	.01	.56	.53	.03	(1) .49	.45	.06	.55	.59	.04
(2)	.42	.44	.02	.47	.53	.06	(2) .51	.48	.03	.57	.59	.02
(3)	.49	.49	.00	.61	.61	.00	(3) .51	.47	.04	.52	.55	.03
T (1)	.28	.41	.13	.48	.53	.05	(1) .49	.48	.01	.60	.62	.02
(2)	.53	.48	.05	.54	.59	.05	(2) .36	.37	.01	.51	.51	.00
(3)	.50	.43	.07	.51	.54	.03	(3) .48	.50	.02	.58	.59	.01

It will also be noticed that predictions of V are equally good for high, medium, and low values of L. For the two highest values of L, 40 and 45 mm., the fit is perfect, while for the lowest value of L, 0.86 mm., the discrepancy is only 1%. With the

Fitting of frequency functions to Urban's lifted-weight results, this JOURNAL, 41, 1929, 70-82. It should be pointed out that while smaller separations than zero are impossible, the theoretical curve actually extends to infinity with negative values. We need not be troubled with negative values of stimulus, since applied mathematics is full of instances where only parts of curves are used having meaning for the problem at hand. On this point see M. Ezekiel, *Methods of Correlation Analysis*, 1930, 59-61.

exception of the 5 cases already mentioned, medium values of L also show excellent agreement between observation and theory.

An even more stringent test of the validity of the $\varphi(\gamma)$-hypothesis in predicting V shows that extrapolated values of V, when V data are not used in the calculations, agree nearly as well with observations as we found above. Omitting the V percentages from the calculation of h and L gave values for the limen and precision factor from which calculated values of V were determined. The differences between actual and observed values did not differ appreciably from those obtained when the observed Vs were included in the calculation of h and L.

The excellent agreement between actual and calculated V shows that judgments of twoness from 0-separation are far from being anomalous.

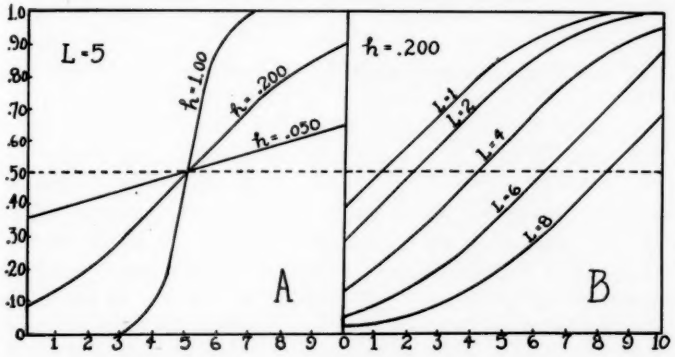

FIG. 1. SHOWING THAT THE NUMBER OF VEXIRFEHLER DECREASES WITH INCREASING VALUES OF h WHEN L IS HELD CONSTANT AS IN A, AND WITH INCREASING VALUES OF L WHEN h IS HELD CONSTANT AS IN B

A rational basis for the occurrence of V is to be found in the $\varphi(\gamma)$-hypothesis itself, making special assumptions, both mathematical and psychological, unnecessary. Since the theoretical percentage of V is a function of γ, decreasing monotonically with γ, it follows that the larger the product of h and L, the smaller will be the expected percentage of *twos* from 0-separation. A higher L, h constant, should yield a smaller V, and a higher h, L constant, should likewise yield a smaller V. These facts are clearly shown in Fig. 1 wherein curves have been drawn for constant L and variable h (Fig. 1 A), and for constant h and variable L (Fig. 1 B). Contrary to the usual assumptions, it follows from this theory that the number of V actually decreases with lowered sensitivity, i.e. where the threshold is higher; and V also decreases with increase in the precision factor, h. If we regard 0-separation merely as the limit of a decreasing series of separations instead of a

stimulus *sui generis,* the rational treatment proposed for V here offers no difficulties.

It follows from what has been said above that we should expect h and V, and L and V, to be correlated negatively. Table III shows this to be true.[9] Since both h and L determine V, we have a right to use the partial r as a truer measure of the correlation between V and either L or h. The value —0.76 for the partial coefficient of correlation between h and V, and L and V, shows that the experimental data exhibit the relationship demanded by the $\varphi(\gamma)$-hypothesis. Owing to the fact that higher L (lower L) may be offset by lower h (higher h), V being determined by the product of h and L, the correlation between V and h or L is not unity. We are justified

TABLE III

SHOWING THE COEFFICIENTS OF CORRELATION (r) BETWEEN L AND h, L AND NUMBER OF VEXIRFEHLER (V), h AND V, AND THE PARTIAL COEFFICIENTS (r)

r		partial r	
L and h	−.40		
h and V	−.45	h and V (L constant)	−.76
L and V	−.45	L and V (h constant)	−.76

in saying, however, that greater precision *tends* to decrease the number of V as well as lesser sensitivity, while lessened precision and greater sensitivity tend to increase V. The negative correlation between L and h shows that increased sensitivity (lowered L) tends to be accompanied by greater precision (higher h), but it shows also that apart from a knowledge of *both h and L* prediction of V is very uncertain due to the independent variability of h and L.[10]

Our criterion of the normality of V consists simply in agreement with the expectancy given by the psychometric function generally used to describe facts of the two-point threshold. Acceptance or rejection of actually observed V rests upon the same basis as the judgments from any other stimulus-value. While traditional treatments of V have always stressed too many V, according to our view there may be *too few* V as well as too many, since poor agreement may be due to the fact that fewer V have been

[9] Riecker concluded from his data that the opposite is true, *i.e.* higher threshold values are accompanied by larger V. Calculation of the coefficient of correlation for his values of L and V giving an r of —.28 shows he misinterpreted his own data. A. Riecker, Versuche über den Raumsinn der Kopfhaut, *Zsch. f. Biol.,* 10, 1874, 177-201.

[10] Müller, who advocated inclusion of V under the $\varphi(\gamma)$-hypothesis, did not see that h and L, by varying independently, might give rise to any theoretical V. He assumed that, the finer the 'place sense,' the smaller would the product hL (γ) be. But h and L may vary in opposite manner, as our negative correlation shows, or together, so that we must always take both into consideration in attempting to predict V.

reported than are demanded by theory. Such a case is found in Table I (*F* on the forehead) where one of the 5 poor fits is due to too few observed V. According to theory, *O* should have reported more *twos* with 0-separation than was actually done in this case. No arbitrary limit can, therefore, be set to the number of V in experimental results except where there is disagreement between observation and theory.

Theoretical and concluding discussion. Inclusion of single-point stimulation offers several advantages: (1) it acquaints *O*s with tactile *oneness* since 0-separation, on any hypothesis, should yield a large number of *one*-reports than any other stimulus value; (2) it conduces to a steadier attitude when even trained *O*s know that their judgments of *one* may be due to single-point stimulation; (3) it tests the *O* at a crucial stimulus-value, thereby furnishing a valuable clue to the goodness of experimental data, since extreme values will yield good agreement with theory under optimal experimental conditions; and (4) when the percentage of V is included in the calculation of h and L, these values are more truly representative of the facts, for, if V is too large, h will be lowered, indicating a lower precision factor, and L will be decreased, indicating that a larger number of twos has been reported, in accordance with experimental facts, than would have been the case had V been omitted from the calculation of h and L.

It will thus be seen that while the use of the single-point stimulus is advocated for the reasons just given, the use of the percentage of twos from this stimulus in the calculation of h and L is optional. If L is found in preliminary experiments to be so large that inclusion of 0-separation would destroy the symmetry of the stimulus-separations, then the V percentage need not be used in the calculation of h and L, provided that the extrapolated percentage for V obtained from the h and L so calculated agrees well with the observed V. We have usually found it possible to choose the stimuli so that 0-separation did not affect the symmetry of the group. Even though the step from O to the smallest separation is larger than that between the other stimuli, it is still possible to include the V percentage by appropriate choice of the x-values in using Urban's method. Where there is bad agreement between calculated and observed V either the data should be discarded and new experiments performed, or the percentage of V should be included in the calculation of h and L in order that the unfavorable value[11] shall have its effect on h and L as well as the 'normal' percentages.

[11] If one calculates Riecker's data given by Titchener, including V, one gets an L of 1.81 and h equal to 0.39, whereas if V is omitted L becomes 2.07 and h increases to 0.48. Titchener omitted V because of the inversion at 0-separation but in so doing

It follows from the theory of V presented here that there is no *a priori* reason why single-point stimulation should yield an impression of oneness in all or even a majority of the stimulations. If h is sufficiently low or L is very small, the psychophysical mechanism should yield a majority of two-judgments for single-point stimulation if the phi-gamma function is an adequate mathematical description of the facts of the threshold. Discrepancies between calculated and observed values of V should, on this view, be regarded in the same way as off values are regarded in other parts of the stimulus continuum, *e.g.* as inversions, or as indicating lack of training, or lack of proper experimental conditions. Thus V will be brought under the rubric of the probability of any psychometric judgment and need no longer be regarded as 'catch-errors,' anomalous, or *Vexirfehler*.

In advocating the subsumption of V to the $\varphi(\gamma)$-hypothesis, justified by experimental and theoretical considerations, we do not mean to imply that this hypothesis is the only possible formulation of the facts of the threshold. On the other hand, we do maintain that any theory of the two-point threshold should include the case of single-point stimulation without special assumptions. If a better theory of the limen is proposed it must predict V better as well as judgments from the other stimulus-values. The fact that the $\varphi(\gamma)$-hypothesis gives such good agreement between calculated and observed values of V leads to the belief that a unified physiological theory of the limen may be worked out in which no special psychological or neurological assumptions are necessary for *Vexirfehler*. Since a single mathematical description of two-point judgments has been found adequate for all the facts we may now look forward to a single physiological theory which needs no special mechanisms to explain the two judgments from single-point stimulation.

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raised the precision factor which indicates a higher reliability than the original data possessed. E. B. Titchener, *Experimental Psychology, Students Manual, Quantitative*, II, i, 1905, p. 94.

THE AFFECTIVE VALUES OF COLOR COMBINATIONS

By CH'UAN-FANG LO, University of Chicago

The purpose of this experiment is fourfold: (1) to study whether the affective values of binary color-combinations are a function of the spectral positions (or approximate wave-lengths) of the components; (2) to test whether the pleasantness of a color-combination is determined by the pleasantness of the individual constituents; (3) to compare the affective reactions of several groups of people with varied amounts of art-training or art experience; and (4) to examine sex and racial differences in color-combination preferences.

METHOD

Materials. In this experiment, 18 Milton-Bradley colors were chosen as approximates to spectral colors. It is acknowledged that actually the pigmentary colors are less pure than the spectral colors. The colors of the Milton-Bradley Pure Spectrum Scale are: violet-red, red, orange-red, red-orange, orange, yellow-orange, orange-yellow, yellow, green-yellow, yellow-green, green, blue-green, green-blue, blue, violet-blue, blue-violet, violet, and red-violet. Of these 18 colors, violet-red and red-violet are non-spectral, but are included to complete the Milton-Bradley color circle. The given approximate wave-lengths for the Milton-Bradley Standards are: red 6571mμ, orange 6085mμ, yellow 5793mμ, green 5164mμ, blue 4695mμ, and violet 4210mμ.

The colored papers were first cut into strips ½ in. wide and 3 in. long. Two strips of each color were used for every combination, and the strips were arranged alternately to form a simple rectangle, mounted on a card 2 in. x 3 in. with no margin. Since each of the colors was combined with every other color, there were altogether 153 combinations. All the cards were enveloped in cellophane to keep the colored papers clean.

Procedure. The whole set of 153 cards was presented to every *S* under ordinary daylight. The table on which the cards were placed was covered with a piece of black velvet to provide a uniform background. On the table were laid 10 white cards in one horizontal row, numbered from 1 to 10, to represent a scale of 10 steps. *S*'s task was simply to sort and distribute all the color-cards into ten piles. The written directions were as follows:

Here are 153 color-cards. Each card is a combination of two colors. All you have to do is to sort these cards into 10 piles, which form a scale of values from the most unpleasant to the most pleasant color-combinations. In pile 1, for example, put those combinations you dislike most, and in pile 10 those you like best. Think of each color-combination as a playing-card back design, and let your first impression guide you. Only one card should be exposed at a time. When you have sorted all

* Accepted for publication July 24, 1935. This study was directed by Professor L. L. Thurstone.

the cards, you may turn over the cards in each pile to see whether they seem equally pleasant or unpleasant to you. You may then shift any card to any other pile.

The *Ss* were allowed all the time they wished.

Subjects. Altogether, 250 adults served as *Ss* in this experiment. The author acted as *E* throughout the study. The *Ss* were divided into four groups: non-artists, art students, artists, and Chinese non-art students. The *Ss* of the first three groups were American; and there was an equal number of men and women in each group. In the Chinese group there was a predominant number of men. Most of the non-artists were undergraduates and graduate students in the University of Chicago. The art students were from the Art Department of the University of Chicago, the School of the Art Institute of Chicago, the Chicago Academy of Fine Arts, and the American Academy of Art. The artists were from 11 different art studios in downtown Chicago.

Results

For each group, the number of times each color-combination appeared in each of the 10 piles was first registered. The median was then calculated. It was the point at which 50% of the group placed the card. This was taken as the scale value of the color-combination. The scale values of 153 color-combinations as determined from the reactions of four different groups of *Ss* are presented in Tables I and II. More than 65% of the color-cards were placed in the lower five piles by the non-artists (American). Art students placed 60% of the cards in the lower five piles; and artists about 57%. The scale values determined by non-artists range from 0.90 to 6.9. The range for art students is from 1.0 to 7.4, and for artists, from 1.3 to 8.0. These results show that more color-combinations were judged unpleasant than pleasant.

We have plotted 18 curves showing the affective values of color-combinations in relation to the spectral positions of the components. They are not here reproduced, but can be reconstructed from the data given in Tables I and II. When we compare the shapes of all the curves for non-artists, we notice that some have similar shapes, and others are similar in part. The curves which are most similar in shape are those for similar colors. We cannot say that the spectral position of any one color absolutely determines the affective value of a combination of which it is a component, since in a binary combination we have to do with two colors. But the relative affective values of the colors for binary combinations may be estimated by calculating for each color the average scale value of all its combinations. In Table III are listed in order the average scale values of 16 spectral colors when used in binary combinations. The highest affective value belongs to blue combinations, while red combinations have the lowest affective value.

TABLE I

SCALE VALUES OF 153 COLOR COMBINATIONS AS DETERMINED BY AMERICAN AND CHINESE NON-ARTISTS

American non-artists

Chinese non-artists	VR	R	OR	RO	O	YO	OY	Y	GY	YG	G	BG	GB	B	VB	BV	V	RV
VR		0.9	2.1	2.6	2.7	2.7	3.0	3.2	2.5	3.5	3.2	2.9	2.7	2.5	2.3	2.9	1.8	2.3
R	1.2		3.4	3.1	3.3	3.2	3.9	3.6	2.6	3.3	3.3	3.4	3.2	3.4	2.4	2.3	1.6	2.2
OR	2.7	3.1		2.2	2.8	3.8	3.9	4.3	3.5	3.7	4.2	4.3	4.8	5.1	4.8	3.3	3.1	2.7
RO	2.8	3.2	1.9		1.6	4.2	4.1	4.1	3.3	3.7	4.2	4.0	4.5	4.6	4.6	3.7	3.1	2.8
O	3.3	3.4	2.6	0.9		4.0	4.6	4.9	3.1	3.3	4.0	3.5	4.3	5.2	4.0	4.0	3.5	2.9
YO	4.3	4.3	4.6	3.7	3.9		3.2	2.8	1.9	3.3	4.5	4.1	5.3	5.7	4.7	4.1	3.7	3.9
OY	3.3	4.2	5.1	3.5	4.1	2.1		2.1	2.4	3.9	5.3	4.4	5.8	6.4	5.3	4.8	3.9	3.8
Y	4.0	4.4	5.1	4.6	5.1	2.5	0.9		1.9	3.7	5.5	5.1	5.8	5.7	5.5	4.9	4.1	4.6
GY	3.9	4.0	4.8	4.3	3.8	2.3	2.4	1.8		1.7	4.3	3.5	3.7	4.6	4.0	3.7	3.4	3.6
YG	3.8	4.0	5.1	4.0	4.1	4.1	3.8	3.8	1.6		5.5	4.0	4.3	4.4	5.1	4.0	3.9	4.0
G	3.5	3.7	4.7	4.4	4.0	4.2	5.8	5.8	4.7	4.8		3.6	4.4	4.9	4.8	4.4	4.2	4.5
BG	3.6	3.6	3.5	4.0	3.0	4.3	4.3	4.6	4.0	4.1	2.4		5.5	6.2	5.8	4.7	4.3	4.3
GB	3.6	3.4	4.6	3.9	4.1	5.3	5.0	5.2	5.2	5.0	4.6	3.6		6.9	5.8	4.1	3.8	4.3
B	2.9	4.0	5.4	4.8	5.3	5.6	5.2	6.2	5.1	6.1	5.6	5.7	4.6		3.6	4.2	3.2	2.9
VB	2.3	3.2	4.8	4.6	4.9	4.7	5.7	6.1	5.3	6.0	5.6	5.2	4.2	2.5		3.3	3.1	3.8
BV	2.5	3.4	4.0	3.8	4.6	4.5	4.8	5.2	4.6	5.0	4.8	4.6	2.9	3.2	1.8		3.0	2.8
V	1.5	1.8	3.6	3.7	3.8	4.0	4.6	3.9	4.8	5.1	4.6	4.4	3.6	2.6	2.6	1.8		2.8
RV	2.0	2.5	3.3	3.4	3.0	4.1	4.2	4.4	4.4	4.0	5.0	3.7	3.1	2.7	3.2	2.1	1.6	

TABLE II

SCALE VALUES OF 153 COLOR COMBINATIONS AS DETERMINED BY ART STUDENTS AND ARTISTS

Art students

Artists	VR	R	OR	RO	O	YO	OY	Y	GY	YG	G	BG	GB	B	VB	BV	V	RV
VR		1.0	2.5	3.0	2.7	3.2	3.3	3.5	3.7	3.6	2.7	4.5	3.5	2.5	2.8	3.2	2.0	2.8
R	1.3		5.1	4.1	4.6	4.6	4.6	4.6	3.5	2.7	1.9	4.3	3.1	2.4	2.4	2.1	1.5	1.8
OR	2.2	5.3		4.1	4.5	3.7	3.6	3.6	4.2	3.0	1.8	3.4	3.0	2.8	3.6	1.8	2.2	1.6
RO	3.2	6.1	5.0		2.6	5.5	4.6	4.8	4.2	3.8	3.1	4.6	3.6	2.8	3.5	2.8	3.1	2.8
O	2.6	5.2	4.1	2.4		5.2	5.4	5.3	4.8	4.1	3.1	3.6	3.8	3.7	4.1	3.0	2.8	2.1
YO	3.3	3.0	3.2	4.0	4.8		4.3	4.9	3.2	4.0	5.0	5.1	4.9	5.0	6.0	4.7	3.5	3.6
OY	3.0	4.0	2.4	4.6	5.2	6.2		3.3	4.3	5.5	5.0	5.3	5.2	5.3	5.0	5.6	3.9	3.4
Y	4.2	4.1	3.3	5.1	5.6	6.5	5.0		3.8	6.3	6.0	5.8	5.4	5.3	4.8	4.6	4.3	4.0
GY	4.1	4.2	2.9	2.9	4.7	4.0	5.4	5.3		2.9	5.4	5.3	3.8	5.0	5.2	5.1	4.2	4.4
YG	5.2	2.5	2.3	3.1	3.8	4.6	4.7	5.1	4.8		7.4	6.7	5.8	5.8	5.2	4.9	4.1	3.0
G	2.6	2.7	1.8	1.8	2.2	3.9	4.2	6.3	6.0	7.3		4.0	4.7	5.0	5.4	4.5	3.8	3.3
BG	6.2	4.2	1.9	4.3	2.4	5.0	5.0	6.0	5.2	7.8	4.3		7.3	7.4	7.0	6.1	6.1	5.2
GB	4.1	2.4	2.5	3.1	2.6	5.0	5.9	6.3	5.6	6.8	4.8	8.0		7.3	5.8	4.8	4.9	4.1
B	3.0	2.1	2.2	2.4	2.6	5.0	5.3	4.6	4.4	5.4	5.5	8.0	7.4		3.8	4.6	3.1	2.8
VB	3.4	2.3	2.4	2.2	2.3	4.5	5.0	5.2	5.6	5.5	5.0	7.3	7.0	3.7		3.5	3.3	4.0
BV	3.1	3.4	2.3	2.4	3.0	4.5	4.6	5.4	4.6	4.9	4.6	6.6	6.8	4.6	4.9		3.7	3.8
V	2.8	1.9	1.7	2.7	2.6	3.8	4.5	5.0	4.1	5.0	4.0	7.1	5.8	4.2	3.2	4.1		3.6
RV	3.6	2.9	2.0	2.2	2.5	3.9	4.2	4.5	4.5	4.4	4.4	6.3	5.8	4.0	5.5	4.4	4.0	

TABLE III

RELATIVE AFFECTIVE VALUES OF SPECTRAL COLORS IN BINARY COMBINATIONS

Color-combination	Av. scale value	Color-combination	Av. scale value
Blue	4.69	Yellow-green	3.86
Green-blue	4.67	Blue-violet	3.79
Green	4.41	Orange	3.66
Blue-green	4.35	Red-orange	3.57
Violet-blue	4.32	Orange-red	3.54
Yellow	4.25	Violet	3.34
Orange-yellow	4.18	Green-yellow	3.18
Yellow-orange	3.90	Red	2.90

Geissler's law of color-combination. In his study of the affective tone of color-combinations, Geissler formulated the following law: "The greater the pleasantness of the individual constituents, the greater will be the pleasantness of the combination."[1] This law has been tested by various investigators,[2] and quite recently Thurstone developed a new mathematical method of testing it which was used in Ofner's study.[3]

Starting upon the assumption that the scale value of a color-combination is due principally to a mere sum of the scale values of the separate components, equations for the derivation of the theoretical scale values of single colors were worked out. These equations are:

$$X_{2.1} = X_1 + X_2 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots [1]$$

in which $X_{1.2}$ represents the scale-value of the combination of colors 1 and 2, and X_1 and X_2 represent the scale-values of the separate components;

$$\Sigma X_{1.k} = (n-1)\ X_1 + \Sigma X_k - X_1 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots [2]$$

in which $\Sigma X_{1.k}$ is the sum of the combination scale-values of color number 1, and ΣX_k is the sum of the separate scale-values;

$$\Sigma X_{k.k} = 2(n-1)\ \Sigma X_k \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots [3]$$

in which $\Sigma X_{k.k}$ is the sum of all the combination scale-values, and ΣX_k is the sum of all the separate scale-values. ΣX is obtained by dividing $\Sigma X_{k.k}$ by $2(n-1)$. When this is known, the scale-values for the separate colors can be derived from Equation [2]:

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[1] L. R. Geissler, The affective tone of color combinations, *Titchener Commemorative Volume,* 1917, 162.

[2] M. F. Washburn, D. Haight, and J. Regensburg, The relation of pleasantness of color combinations to that of colors seen singly, this JOURNAL, 32, 1921, 145-146; M. Yokoyama, Affective tendency as conditioned by color and form, *idem,* 81-107; W. E. Walton and B. M. Morrison, A preliminary study of the affective values of colored lights, *J. Appl. Psychol.,* 15, 1931, 294-303.

[3] Marion Ofner, *The Affective Values of Color Combinations,* M.A. thesis, University of Chicago, 1934.

$$\Sigma X_{1.k} = (n-1)\ X_1 + \Sigma X_k - X_1 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots [2]$$

$$X_1 = (\Sigma_{1,k} - \Sigma X_k)/(n-2).$$

Similarly,

$$X_2 = (\Sigma X_{2,k} - \Sigma X_k)/(n-2),$$

and so on for all the single colors. The theoretical scale-value for the combination of colors 1 and 2 is merely the sum of X_1 and X_2.

The test of Geissler's law is to be found in the degree of agreement between the theoretical scale-values of the color-combinations thus derived, and the experimental scale-values. We have calculated by the same technique the theoretical scale-values of all of the 153 color-combinations. It was found that the discrepancies between the calculated scale-values and the experimental scale-values as determined by non-artists vary a great deal. The mean discrepancy is 0.53. The correlation between experimental scale-values and calculated scale-values is 0.67 ± .03. Geissler's law does not apply to all cases of color-combinations; and it seems dangerous to predict the pleasantness of a color-combination on the basis of the pleasantness of its constituents.

Effect of training. In Table IV are shown the correlations between different groups of *S*s. The correlations are not of the same order, but are all positive, which indicate the presence of some common factor in the

TABLE IV

CORRELATIONS BETWEEN DIFFERENT GROUPS OF SUBJECTS

	Non-artists	Art students	Artists
Non-artists	(.78)	.64± .03	.41± .04
Art students	.64± .03	—	.79± .02
Artists	.41± .04	.79± .02	—

affective reactions of all groups, with or without art training. We may generalize that the amount of agreement in color-combination preferences decreases with increase in the amount of art training in one of the two groups. That people with some art training agree more among themselves than with non-artists is indicated by the relatively high correlation between art students and artists.

Sex and racial differences. In order to compare sex differences in color-combination preferences, the 200 American subjects were re-classified into four groups: men and women without any art training, and men and women with art training. Scale-values for all 153 color-combinations were calculated for each group. Their correlations are presented in Table V. It appears that when both men and women receive art training, differences in color-combination preferences due to sex are much reduced. Two groups which differ in sex and in art training agree less than two groups which differ only in sex. On the whole, the sex differences are not so great as differences due to art training.

Racial differences in color-combination preferences between Chinese and American non-artists are negligibly small. The correlation in scale-values is $0.73 \pm .025$, which is almost as high as the self-correlation for the

TABLE V

CORRELATIONS BETWEEN MEN AND WOMEN

	Men without art training	Women without art training	Men with training	Women with training
Men without art training	—	$.59 \pm .035$	$.48 \pm .042$	$.41 \pm .045$
Women without art training	$.59 \pm .035$	—	$.48 \pm .042$	$.62 \pm .033$
Men with training	$.48 \pm .042$	$.48 \pm .042$	—	$.78 \pm .021$
Women with training	$.41 \pm .045$	$.62 \pm .033$	$.78 \pm .021$	—

American non-artists. But the correlation between Chinese non-artists and American art students is only $0.43 \pm .044$; and the correlation with American artists is as low as $0.19 \pm .052$. Art training, or art experience, apparently has greater influence in such matters as color preference than race or culture.

SUMMARY AND CONCLUSIONS

An experiment on the affective values of 153 binary combinations of 16 spectral and 2 non-spectral colors was performed on 100 American non-artists, 50 art students, 50 artists, and 50 Chinese students without art training. The combinations were presented in the form of color-cards, and the subjects were asked to sort the cards into ten gradations of pleasantness-unpleasantness. The results of the experiment lead to the following conclusions.

(1) All groups of *Ss* found more unpleasant than pleasant color-combinations, but art students and artists reacted more favorably than non-artists.

(2) Colors at the two ends of the spectrum were found to make the most unpleasant combinations. For that portion of the spectrum from red to blue, it may be said with one or two exceptions, that the affective values of the spectral colors in binary combinations increase with decrease in wave-lengths, but beyond the most saturated blue, affectivity decreases. The most favored spectral positions for binary combinations are represented by $blue_2$, green, and yellow. That combinations of the longest and shortest wave-lengths should be judged most unpleasant might be due to the fact that colors at the ends of the spectrum are darker than the other colors. Colors whose spectral positions are close together may produce either pleasant or unpleasant combinations.

(3) The pleasantness of a color-combination is not always a mere sum of the pleasantness of its constituents.

(4) Color-combination preferences between any two groups of people vary according to the amount of art training.

(5) Sex and racial differences in color-combination preferences are not as great as differences due to art training.

(6) A number of factors other than the spectral order of the colors may have been of influence in the determination of the affective values of the combinations. Such factors are color-pattern, purity of colors, proportion, and background. Variations in any of these may be expected to give different results.

CONTRAST EFFECTS AND THE PSYCHOMETRIC FUNCTIONS

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The intra-serial effects were discovered by Fernberger[1] and have been studied by Turner,[2] Arons and Irwin,[3] and Preston.[4] Turner[5] gave a theory which accounts for the direction in which the judgment category changes by assuming a shifting of the effective value of the standard weight as the result of certain stimulus conditions. Preston has criticized the theory on the grounds that it does not account for the increased probabality of occurrence of certain preferred categories of judgment, nor for the characteristic avoidance of the repetition of the category *equal*, nor for contrast effects existing between judgments not temporally proximate.[6]

Besides the methodological problem involved here, Turner's theory is important to the systematic problem of changes in retained visual material with the passage of time.[7] In this respect it could take the place of Gibson's theory,[8] which criticized and replaced Wulf's explanation.[9] Wulf's theory of retentive changes was developed on 'Gestalt' assumptions. In the place of Wulf's theory Turner proposed that the course of retained material, as it may be studied in the judgment errors found in psychophysical experimentation, is determined by events transpiring at or immediately before the time of impression. The evidence, which he took to support this point of view, involves the fluctuations in the psychometric function which are independent of that category of judgment to which the comparison of an immediately preceding pair of weights is referred.[10] Preston showed in part that these fluctuations may be attributed to the effect of the *S*'s attitude.[11]

* Accepted for publication April 9, 1935.

[1] S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 126-150.

[2] W. D. Turner, Intraserial effects with lifted weights, this JOURNAL, 43, 1931, 1-25.

[3] L. Arons and F. W. Irwin, Equal weights and psychophysical judgments, *J. Exper. Psychol.*, 15, 1932, 733-751.

[4] M. G. Preston, Contrast effects and the psychophysical judgments, this JOURNAL, 48, 1936, 389-402.

[5] Turner, *op. cit.*, 24 f.

[6] Preston, *op. cit.*, 389-402.

[7] Turner, *op. cit.*, 22-24.

[8] J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39.

[9] F. Wulf, Ueber die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psychol. Forsch.*, 1, 1922, 333-373.

[10] Turner, *op. cit.*, 16.

[11] Preston, *op. cit.*, 389-402.

There remains the question as to whether other influences may be shown also to operate in the experimental series used by him and whether these influences may be interpreted in accordance with Turner's theory. The present experiment proposes to investigate the effect of comparing equal pairs of weights on the psychometric functions derived from a standard psychophysical series. The experiment is comparable to Turner's "inter-pair" experiment but not to his "intra-pair" study. By the elimination of extreme values the procedure reduces to a minimum whatever effects are due to stimulus conditions.

Procedure. Two series of weights were used, Series B and Series C. Series B consisted of 28 aluminum cylinders, surrounded on the lifting surface by a felt pad. Fourteen of them served as standard weights at 100 gm. and the other fourteen served as comparison weights, distributed two each at intervals of 4 gm. from 84 to 108 gm. The comparison weights were arranged in a random order, but in each even numbered sitting the order of comparison weights was an exact inversion of the order in the preceding odd-numbered sitting. A standard weight preceded each comparison weight on the table.

Series C consisted of 28 aluminum cylinders of form identical to those described above. Twenty-one of these cylinders weighed 100 gm., and the remaining seven (referred to hereafter as variable weights) were distributed one each at intervals of 4 gm. from 84 to 108 gm. The stimuli were arranged in the following manner. Each weight of the seven in the series from 84 to 108 served as a comparison weight. It was preceded on the table by its standard of 100 gm. and an additional pair of 100 gm. weights. The first of this pair was a standard and the second a comparison weight. When this series was presented to the *S* he compared first a 100 gm. weight with a 100 gm. weight, then he compared a variable weight against a 100 gm. weight. Any given order of variable weights was used four times before it was changed. No one arrangement was used on consecutive days. At the beginning of each even numbered set of four sittings on variable weights, the order of the weights was changed by inverting the order used during the preceding odd numbered set of four sittings.

All comparisons were recorded by the *E* in such a way that, while an exact record was kept of each judgment made in its proper temporal position, the judgments on variable weights were classified immediately according to the category of the judgment used in the preceding pair of equal weights.

All weights were presented to the *S* at a rate of 92 beats per min. on a freely moving turn-table. They were lifted consecutively in rhythm with an electrically driven metronome. A screen separated the *S* from the turn-table. The weights were checked on a chemical balance before, during and after the experimentation and were found to be constant within the limits of 10 mgm. Experimentation was begun in May, 1933, was concluded in June, 1934, and was carried on in the Psychological Laboratory at the University of Pennsylvania.

The *Ss* in this experiment were: Esther Israeli (*Is*), W. R. Carlson, (*Ca*), M. W. Mays (*M*), J. F. Slack (*Sl*) and A. G. Rohs (*R*). Of the *Ss* only *Ca* had had previous experience in lifting weights.

Results. Two series of controls were used in this experiment. The first control series was Series B. The second and more important control series consisted of all remaining judgments on Series C after those judgments

TABLE I

PSYCHOPHYSICAL CONSTANTS ON ALL EXPERIMENTAL AND CONTROL SERIES

S	Series	S_1	S_2	h_1	h_2	PSE	IU
	B	94.80	100.48	.1105	.1074	97.60	5.68
	C_1	97.39	101.57	.1209	.1158	99.43	4.18
	C_{eg}	96.79	100.47	.1243	.1278	98.65	3.68
Ca	C_e	97.98	99.12	.1266	.1291	98.55	1.14
	C_{1g}	96.54	101.53	.1240	.1234	99.03	4.99
	C_g	96.08	101.24	.1124	.1330	98.86	5.16
	C_{1e}	97.66	100.79	.1280	.1144	99.14	3.13
	B	85.01	101.31	.0789	.1034	94.20	16.30
	C_1			Data Insufficient			
	C_{eg}			Not Applicable			
R	C_e	87.45	100.99	.1011	.1074	94.42	13.54
	C_{1g}	86.91	103.22	.1032	.0940	94.68	16.31
	C_g	86.94	103.33	.1028	.0801	94.12	16.39
	C_{1e}	87.52	101.09	.1018	.1062	94.45	13.57
	B	95.32	97.84	.1109	.1026	96.53	2.52
	C_1	97.20	98.08	.0999	.1063	97.65	.88
	C_{eg}	96.84	97.73	.1137	.1136	97.29	.89
M	C_e	95.84	97.00	.0745	.0826	96.45	1.16
	C_{1g}	96.96	97.75	.1124	.1135	97.35	.79
	C_g	96.88	97.68	.1161	.1149	97.28	.80
	C_{1e}	96.99	98.13	.0962	.1040	97.58	1.14
	B	94.10	97.91	.0817	.0798	95.99	3.81
	C_1	95.11	97.68	.0894	.0961	96.44	2.57
	C_{eg}	95.39	97.74	.0914	.0893	96.49	2.35
Is	C_e	95.15	96.17	.0917	.0916	95.66	1.02
	C_{1g}	95.32	97.88	.0895	.0920	96.62	2.56
	C_g	95.43	97.93	.0899	.0898	96.68	2.50
	C_{1e}	95.11	97.25	.0902	.0965	96.22	2.14
	B	89.52	100.96	.0766	.0716	94.63	11.44
	C_1	89.28	99.33	.0673	.0693	94.38	10.05
	C_{eg}	90.23	100.44	.0734	.0669	95.10	10.21
Sl	C_e	91.58	97.05	.0694	.0691	94.31	5.47
	C_{1g}	89.70	101.83	.0750	.0718	95.63	12.13
	C_g	89.74	102.61	.0756	.0734	96.08	12.87
	C_{1e}	90.82	97.32	.0672	.0593	93.87	6.50

which followed a given judgment had been abstracted from the total distribution on Series C.

Table I gives the values of the psychophysical constants calculated by the constant process with the use of Urban's tables[12] for all *S*s and for

[12] F. M. Urban, Hilfstabellen für die Konstanzmethode, *Arch. f. d. ges. Psychol.*, 24, 1912, 236-243.

all series, both experimental and control. The various series are enumerated in the first column of Table I. The key to the symbols follows:

C_l = all judgments in Series C made after judging equal weights *lighter*.
C_e = all judgments in Series C made after judging equal weights *equal*.
C_g = all judgments in Series C made after judging equal weights *greater*.
C_{lg} = all judgments in Series C except those made after judging equal weights *equal*.
C_{le} = all judgments in Series C except those made after judging equal weights *greater*
C_{eg} = all judgments in Series C except those made after judging equal weights *lighter*.
S_1 = the threshold for the *lighter* category with variable weights.
S_2 = the threshold for the *greater* category with variable weights.
h_1 = the measure of precision for the *lighter* judgments with variable weights.
h_2 = the measure of precision for the *greater* judgments with variable weights.
IU = the interval of uncertainty.
PSE = the point of subjective equality.

In order to clarify this table, it has been summarized in Tables IIA, IIB, and IIC, which show the magnitude of all psychophysical constants relative to their controls. Table IIA summarizes the constants computed for the

TABLE IIA

RELATIVE MAGNITUDES OF PSYCHOPHYSICAL CONSTANTS IN CONTROL SERIES WHEN COMPARED WITH EXPERIMENTAL SERIES FOR DATA TAKEN AFTER JUDGING EQUAL WEIGHTS *Lighter*

S	S_1	h_1	S_2	h_2	PSE	IU
Sl	$C_{eg}>B>C_l$	$B>C_{eg}>C_l$	$B>C_{eg}>C_l$	$B>C_l>C_{eg}$	$C_{eg}>B>C_l$	$B>C_{eg}>C_l$
Ca	$C_l>C_{eg}>B$	$C_{eg}>C_l>B$	$C_l>B>C_{eg}$	$C_{eg}>C_l>B$	$C_l>C_{eg}>B$	$B>C_l>C_{eg}$
Is	$C_{eg}>C_l>B$	$C_{eg}>C_l>B$	$B>C_{eg}>C_l$	$C_l>C_{eg}>B$	$C_{eg}>C_l>B$	$B>C_l>C_{eg}$
M	$C_l>C_{eg}>B$	$C_{eg}>B>C_l$	$C_l>B>C_{eg}$	$C_{eg}>C_l>B$	$C_l>C_{eg}>B$	$B>C_{eg}>C_l$
R	No Data					

judgments made on variable weights after judging equal weights *lighter*. Tables IIB and IIC summarize the constants for variable weights after judging equal weights *equal* and *greater* respectively. The columns are arranged like those of Table I. In Table IIA under S_1 for *Sl* the table must be interpreted as meaning that, in the case of *Sl*, the threshold computed from data in Series C_l on lighter judgments was lower than that calculated from data in series B which in turn was lower than that computed on data from Series C_{eg}.

Summary of results. Inspection of the results as shown in Tables I and II shows that little uniformity of change in the psychophysical values is present. However, the following generalizations may be made:

(1) Precision for the *lighter* judgments decreases when the judgment follows a judgment *lighter*.

(2) After a judgment *equal* on equal weights, the threshold for *heavier* decreases, the PSE decreases, and the IU decreases.

(3) After a judgment *greater* on equal weights, the threshold for *lighter* decreases, the threshold for *greater* increases and the IU increases.

Discussion. The explanations of the contrast effects, in terms both of attitude and in terms of the shifting value of the standard weight, agree with respect to the expectation of certain of the changes found in the psychophysical constants. After a judgment *lighter* both theories predict a decrease in the number of *lighter* judgments and an accompanying decrease in the threshold for *lighter*. After a judgment *greater* both theories predict a decrease in the number of *greater* judgments and an accompanying increase in the threshold for *greater*.

TABLE IIB

SAME AS TABLE IIA, EXCEPT THAT DATA ARE TAKEN AFTER JUDGING EQUAL WEIGHTS *Equa*

S	S_1	h_1	S_2	h_2	PSE	IU
Sl	$C_e > C_{lg} > B$	$B > C_{lg} > C_e$	$C_{lg} > B > C_e$	$C_{lg} > B > C_e$	$C_{lg} > B > C_e$	$C_{lg} > B > C_e$
Ca	$C_e > C_{lg} > B$	$C_e > C_{lg} > B$	$C_{lg} > B > C_e$	$C_e > C_{lg} > B$	$C_{lg} > C_e > B$	$B > C_{lg} > C_e$
Is	$C_{lg} > C_e > B$	$C_e > C_{lg} > B$	$B > C_{lg} > C_e$	$C_{lg} > C_e > B$	$C_{lg} > B > C_e$	$B > C_{lg} > C_e$
M	$C_{lg} > C_e > B$	$C_{lg} > B > C_e$	$B > C_{lg} > C_e$	$C_{lg} > B > C_e$	$C_{lg} > B > C_e$	$B > C_e > C_{lg}$
R	$C_e > C_{lg} > B$	$C_{lg} > C_e > B$	$C_{lg} > B > C_e$	$C_e > B > C_{lg}$	$C_{lg} > C_e > B$	$C_{lg} > B > C_e$

TABLE IIC

SAME AS TABLE IIA, EXCEPT THAT DATA ARE TAKEN AFTER JUDGING EQUAL WEIGHTS *Greater*

S	S_1	h_1	S_2	h_2	PSE	IU
Sl	$C_{le} > C_g > B$	$B > C_g > C_{le}$	$C_g > B > C_{le}$	$C_g > B > C_{le}$	$C_g > B > C_{le}$	$C_g > B > C_{le}$
Ca	$C_{le} > C_g > B$	$C_{le} > C_g > B$	$C_g > C_{le} > B$	$C_g > C_{le} > B$	$C_{le} > C_g > B$	$B > C_g > C_{le}$
Is	$C_g > C_{le} > B$	$C_{le} > C_g > B$	$C_g > B > C_{le}$	$C_{le} > C_g > B$	$C_g > C_{le} > B$	$B > C_g > C_{le}$
M	$C_{le} > C_g > B$	$C_g > B > C_{le}$	$C_{le} > B > C_g$	$C_g > C_{le} > B$	$C_{le} > C_g > B$	$B > C_{le} > C_g$
R	$C_{le} > C_g > B$	$C_g > C_{le} > B$	$C_g > B > C_{le}$	$C_{le} > B > C_g$	$C_{le} > B > C_g$	$C_g > B > C_{le}$

On the other hand, the two theories differ with respect to their ability to predict what will happen after a judgment *equal* and with respect to what they imply about changes in the limens for *lighter* after a judgment *greater*, and *greater* after a judgment *lighter*. Turner's theory requires an increase in the threshold for *lighter* after a judgment *greater* together with an increase in the PSE. After a judgment *equal*, Turner's theory makes no prediction; but the attitude theory predicts an increase in the size of the IU. After a judgment *lighter*, both theories expect a decrease in the threshold for *lighter*, while Turner's theory in addition implies a decrease in the size of the limen for *greater*.

That these results do not support the theory of a shifting effective value of the comparison weight nor the deductions therefrom as a complete explanation of the course of retained memorial data is evident. In one

respect both of the theories are deficient: the lack of suitable changes after a judgment *lighter* cannot be explained by either. This fact is not entirely unexpected, however, because a previous study[13] indicated that the judgment *lighter* has the least effect upon a subsequent judgment. On the other hand, the changes which take place after a judgment *equal* definitely fall outside the possibility of explanation by Turner's theory and the changes occurring after a judgment *greater* definitely contradict that theory. Furthermore, if the disposition against repetition of judgment be disregarded for a moment, the remaining effects, as they appear in the psychometric functions and as attributable to judgment preferences, are similar to the effects associated with the time-error of judgment. After judging equal weights *equal*, for example, we find a definite decrease in the value corresponding to the PSE together with a definite decrease in the limen for *greater*. After judging equal weights *greater*, the limen for *lighter* decreases while the IU increases. This latter fact indicates an increase in the number of *equal* judgments, *i.e.* in the category which includes those judgments which are greatest in magnitude and available to the *S* when his attitude momentarily prevents the use of the *greater* category. We may conclude that, no matter what category of judgment is used in the comparison of a previous pair for which the second stimulus is not extreme in value, the standard weight of the second pair loses its effective value in its relation to its comparison weight, provided the effects on the psychometric function attributable to judgment avoidance are discounted. It appears therefore that the contrast effects, as they appear in the results of the application of the equal-weight technique and in the results of the usual psychophysical experiment (when no extreme stimuli are present in the first pair of the two under consideration), may be explained on the basis of a prejudice (attitude) of the *S* against the repetition of judgments, and a secondary influence resembling the well-known tendency to overestimate the intensity of the second of two stimuli with respect to the first.

In respect of the complete explanation of the contrast effects as Fernberger first found them, it appears that at least three sets of conditions are at work to produce the effects. Two of the three are disclosed here and the third arises from the stimulus conditions which Turner studied. In respect of the problem of retention, it is evident, in so far as the study of judgment errors is pertinent, that the introduction of extreme stimuli serves to change a one-directional change in the effective value of the standard weight into a two-directional change. The direction of the change

[13] Preston, *op. cit.*, 389-402.

in this latter case depends on the qualitative nature of the extreme stimulus preceding the standard weight.

Conclusions

(1) Psychophysical values derived from three series, one of which was preceded by judgments *lighter* on equal weights, another by judgments *equal* on equal weights, and the third by judgments *greater* on equal weights, were studied comparatively.

(2) After a judgment *equal* on equal weights, the time-error increases.

(3) After judgment *greater* on equal weights, the interval of uncertainty increases and the limen for *lighter* decreases.

(4) Contrast effects following pairs of equal weights may be explained by the assumption of an attitude against repeating judgments on the part of the *S* together with the operation of a factor similar to the negative time-error.

THE CHRONAXY OF PRESSURE AND PAIN

By WALTER S. NEFF and KARL M. DALLENBACH, Cornell University

In 1923, two decades after Lapicque's pioneer study of motor chronaxy,[1] Bourguignon made the first determination of sensory chronaxy,[2] stimulating the cutaneous nerves directly.[3] Further determinations of cutaneous chronaxy were reported by Stein in 1930,[4] by Walthard and Weber,[5] and by Ruffin in 1932;[6] all of whom stimulated the cutaneous nerves in the normal way, *i.e.* through the end-organs.

Except for Walthard and Weber, these investigators, ignoring the long established differentiation of cutaneous sensitivity,[7] fail to specify modality. Without such specification, one cannot tell whether their chronaxies apply to pressure or pain, and so relate their findings to the general topic of cutaneous sensitivity.

Walthard and Weber made an attempt at differentiation. Following the common clinical method, they mapped the skin by working over it with a horse hair for pressure spots and with a fine needle for pain spots. They then obtained higher chronaxies from the pain spots than from the pressure spots. These determinations, however, were rendered doubtful by the surprising fact that the observers reported the same quality—a light, dull pressure—both for pain and pressure spots,[8] a fact that points to some error in procedure.

The purpose of the present study is to verify the difference in chronaxy between pressure and pain under carefully controlled conditions.

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[1] Cf. L. Lapicque, *L'excitabilité en fonction du temps: La chronaxie, sa signification et sa mésure*, 1926. 1-371.

[2] G. Bourguignon, *La chronaxie chez l'homme*, 1923, 1-418.

[3] This is indicated by the fact that his subjects reported a tingling (picotement) that proceeded down the arm from the point of stimulation, following the path of the nerve fiber (198 ff.).

[4] H. Stein, Untersuchungen über die Chronaxie der Sensibilität, *Dtsch. Zsch. f. Nervenhk.*, 100, 1930, 221-223.

[5] K. M. Walthard and A. Weber, Zur Chronaximetrie der normalen Hautsensibilität des Menschen, *Zsch. f. d. ges. Neurol. u. Psychiat.*, 140, 1932, 67-88.

[6] H. Ruffin, Chronaximetrische Untersuchungen des sensiblen und optischen Apparates, *ibid.*, 140, 1932, 641-656.

[7] M. Blix, Experimentelle Beiträge zur Lösung der Frage über die specifische Energie der Hautnerven, *Zsch. f. Biol.* 20, 1884, 141-156; A. Goldscheider, Ueber Wärme-, Kälte-, und Druckpunkte, *Arch. f.* [*Anat. u.*] *Physiol., Physiol. Abt.*, 1885, 340-345; H. H. Donaldson, On the temperature sense, *Mind*, 10, 1885, 399-416; M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. kgl. sächs. Ges. d. Wiss., math.-phys. Kl.*, 47, 1895, 181-184.

[8] Walthard and Weber, *op. cit.*, 74.

METHOD AND PROCEDURE

We selected five *Os* with training in cutaneous observation and gave them additional training under the particular conditions of our experiment.[9] With each *O* we obtained three strength-duration curves for pain and pressure from three areas on the dorsal surface of the right forearm for a wide range of voltages varying at

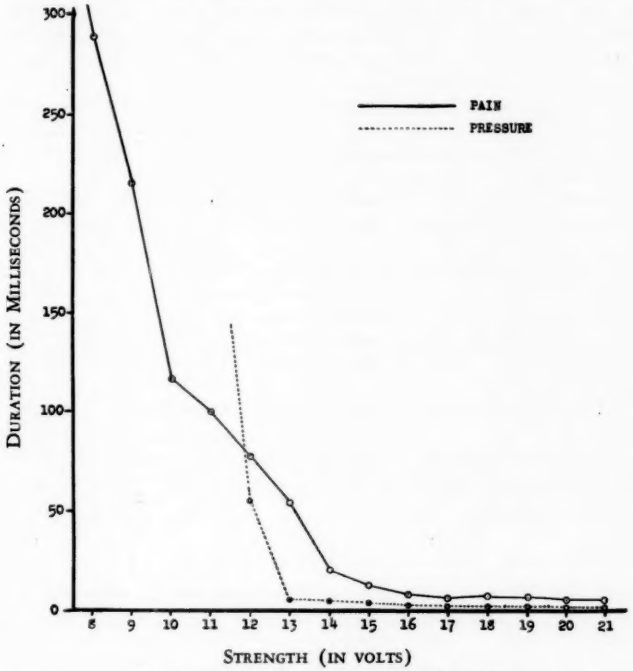

FIG. 1 STRENGTH-DURATION CURVES FOR *Da*

1-volt intervals and for durations varying from a few milliseconds up to several hundred milliseconds.[10]

The instructions used in establishing the strength-duration curves were as follows:

[9] The observers were Dr. G. D. Glanville (*Gl*), Merriam fellow in psychology; Dr. H. J. P. Schubert (*Sc*), National Research Council fellow in psychology; Miss M. B. Erb (*Er*) and Mr. A. D. Freiberg (*Fr*), graduate students in psychology; and the senior author (*Da*). The junior author served as the *E* throughout the study and also made first-hand observations during the preliminary experiments.

[10] At first some difficulty was encountered in obtaining rheobase values for pressure. If the duration of current flow was unlimited, the density was so great that pain alone was reported. When the rheobase durations were reduced to about 100 ms., which is still well above physiological 'infinity,' pressure was easily perceived.

You will be given a series of stimulations, some of which will arouse a sensory quality and some of which will not. Report your experiences and specify their modality. You will be given a "ready-now" signal before every stimulation.

Fig. 1 shows a typical pair of strength-duration curves for *Da,* obtained by plotting current-strength (in volts) on the abscissa against duration of flow (in ms.) on the ordinate. The most important feature of these curves is the relationship that they present between pain and pressure as functions of time and current-strength. Within a certain range of voltages, no matter how long the duration, only pain is aroused. Once the voltage is high enough to arouse pressure, a shorter duration is necessary for its arousal than is necessary for the arousal of pain.

In the determination of the chronaxies of pressure and pain, the dorsal surface of *O*'s right forearm was divided into five approximately equal areas. Determinations for any one area were never made consecutively and never more than two determinations were made on a given area in a single experimental hour.[11]

We used the improved thyratron-tube chronaximeter recently described,[12] and non-polarizable electrodes. The negative electrode was a ¼ in. glass tube with a platinum wire sealed through one side. The tube was filled with physiological salt-solution and one end was drawn to a point. From this point, a cotton wick projected 2-3 mm., and made contact with the skin. The positive electrode was a sheet of copper screening (2 x 6 in.) sewed into a cheese-cloth bandage which was saturated with a physiological salt-solution and tied firmly around *O*'s left arm. A by-pass of known resistance was inserted into the chronaximeter when long stimulus-durations were required.

The instructions used for the determination of the chronaxy were as follows:

You will be given a series of stimulations. Report the first which arouses a pressure [pain] quality. After a short pause, stimulations will begin again. Again report the first pressure [pain] quality. You will then be allowed to rest a moment while the position of the electrode is changed. You will be given a "ready-now" signal before every stimulation.

Results

Table I shows the average chronaxies for pressure and pain and the S.D. of the averages. The values represent 60 determinations with every *O. Only*

[11] In the standard method of chronaxy, as developed by Lapicque and his co-workers, excitability of tissue is measured in terms of two variables, the strength of the stimulating current (D.C.) and the duration of its application. One first determined the strength of current that just suffices (when applied for a duration that is physiologically infinite, *e.g.* 1/5 sec.) to excite the tissue in question. This strength, called the rheobase or rheobase-voltage, is then doubled—presumably to bring the strength of the stimulating current to a point that falls where the curve of excitability is changing most rapidly. The double rheobase current, kept at a constant strength, is passed through the tissue in successive trials for variable lengths of time—from zero duration to longer and longer intervals until the first evidence of excitation appears. The duration of a current of double rheobase strength that just suffices to excite a tissue is called the chronaxy of that tissue. This chronaxy is taken by many to be a measure of excitability, and is correlated with such things as speed of conduction, diameter of nerve-fiber, and effect of toxins.

[12] G. Kreezer, J. D. Coakley, and K. M. Dallenbach, A thyratron chronaximeter: Its operation and calibration, this JOURNAL, 46, 1934, 641-647.

those trials are included in our computations in which the same quality was reported as in the determination of the rheobase. It will be seen that the average pressure-chronaxy is of the same order of magnitude for all the *Os*, and that the differences among the averages (cf. Table II) are unreliable. These results imply a remarkable similarity in function in three different organisms.

Table I also shows the general trend of the pain-chronaxies. They are considerably larger than the pressure chronaxies. In the case of *Er*, the

TABLE I

AVERAGE CHRONAXIES OF PRESSURE AND PAIN

(Values in milliseconds)

O	Pressure		Pain	
	Av.	$\sigma_{av.}$	Av.	$\sigma_{av.}$
Sc	0.133	0.031	—	—
Er	0.106	0.0295	0.278	0.033
Gl	0.122	0.0290	—	—
Fr	—	—	0.453	0.045

difference in chronaxy between pressure and pain is 0.172 ms., and the reliability ($D/\sigma_{diff.}=4.8$) of that difference approaches mathematical certainty. This important result demonstrates a real temporal difference in excitation between pressure and pain.

The strength-duration curves reveal certain peculiarities in the excitation of pressure and pain. Let us assume, for illustration, that we are stimulating the forearm with voltages from zero up, each pulse of current

TABLE II

RELIABILITY OF DIFFERENCES BETWEEN *Os* IN AVERAGE PRESSURE CHRONAXIES

(Values in milliseconds)

Os	D	$\sigma_{diff.}$	$D/\sigma_{diff.}$
Sc—*Er*	0.027	0.034	0.8
Sc—*Gl*	0.011	0.033	0.3
Gl—*Er*	0.016	0.032	0.5

physiologically infinite in duration. At a certain voltage *O* reports a faint, sharp pain. We continue stimulating, increasing voltage by small steps. *O* continues to report pain, noting increases in its intensity. Suddenly, he reports a new quality—a dull, very brief pressure, whose latent period is shorter than that of pain, so that from now on two qualities are present, pain appearing an instant after pressure. This situation is typical of rheobase determinations (cf. Fig. 1). In chronaxy determinations the voltage is already so high (double the rheobase) that the only influential variable is

duration. Now, when we increase the duration from zero, the first quality reported is not pain but pressure, and we must increase the duration considerably above the point arousing pressure to arouse pain (cf. Fig. 1). In normal skin, therefore, the rheobase would be lower, and the chronaxy higher, for pain than for pressure.

With the exception of Walthard and Weber, the earlier investigators say very little about their procedure. From incidental remarks and from our knowledge of the general method of chronaxy, however, we can safely reconstruct their method. The rheobase was obtained by increasing current-strength from zero by small steps, at a constant infinite duration, until the first report was made. This would be, as we have just shown, a report of faint sting or pain. Doubling the voltage used at this point, the experimenter would then apply the voltage for durations ranging from zero upwards, again stopping with the first report. In this case, however, the first quality reported would not be pain, as in the rheobase trial, but pressure (cf. Fig. 1). Our analysis of the procedure is supported by Walthard and Weber, who tell us that the quality reported in the determination of chronaxy for a pain spot was a short, dull flick (*Schlag*), although the rheobase was painful.[13]

Our predecessors, then, obtained pressure chronaxies with rheobases suitable only for pain, and they give us values for pain chronaxy where the chronaxy quality is pressure. To avoid such confusion, we insisted, in our experiment upon *identity of quality at both rheobase and chronaxy determination.*

Under our conditions, lower chronaxies are to be expected for pressure than in the older experiments. The expectation is confirmed. Values for cutaneous chronaxy given in the literature (where modality is not indicated) cluster about 0.250 ms.; our averages for pressure range from 0.106 to 0.133 ms. Our values for chronaxy of pain are much larger, ranging from 0.278 to 0.453 ms.

Our strength-duration curves, taken together with our chronaxy values, give a complex picture of pain and pressure sensitivity. The two variables of voltage and duration have different effects. Under variable voltage, pain shows a higher absolute sensitivity than pressure. On the other hand, when duration is variable and within a certain range of voltages, pressure displays a higher sensitivity than pain. Pain is aroused by weak currents applied for long durations, pressure by stronger currents applied for shorter durations. This conclusion, combined with our discovery that pain shows a longer

[13] Walthard and Weber, *op. cit.*, 74.

latent period than pressure (cf. p. 635), provides additional support for a disparity of morphological structures between the modalities of pressure and pain.[14]

SUMMARY

(1) We have measured the chronaxy of pressure and pain.

(2) Chronaxy is lower for pressure than for pain, the differences between the averages of the two chronaxies being highly reliable.

(3) The chronaxy-times for pressure range from 0.106 to 0.133 ms., for pain from 0.278 to 0.452 ms.

(4) We have obtained strength-duration curves for pressure and pain which show a higher absolute sensitivity for pain under variable voltage, and for pressure under variable duration above a certain point.

(5) Our insistence upon identity of report in the determination of both rheobase and chronaxy, accounts for the discrepancies between our results and those of earlier investigators.

[14] For other evidence see B. Edes and K. M. Dallenbach. The adaptation of pain aroused by cold, this JOURNAL, 48, 1936, 307-315, and the references cited by them.

SIZE-CONSTANCY OF THE PROJECTED AFTER-IMAGE

By HARRY HELSON, Bryn Mawr College

Under the usual conditions of observation the size of the negative after-image varies directly with the distance of projection in accordance with Emmert's law: $I/O = D/d$, where I and O are linear dimensions of image and stimulus-object, and D and d are the distances of projection and fixation, respectively. In this paper it will be shown that when cues to distance and surroundings are eliminated the *apparent size* remains practically constant while the *measured size* of the projected image tends to obey Emmert's law. This surprising result will be explained by factors commonly neglected in treatments of size-constancy and will furnish the basis for the argument that no justification exists for assuming that apparent size conforms to measured size under all conditions. The explanation advanced here for size-constancy of the after-image will also suffice to explain other anomalies of apparent size without recourse to the abstract and highly artificial processes assumed by some writers on this subject.

Procedure. Five individuals, three undergraduates and one graduate student at Bryn Mawr, and the writer, acted as *Os* after preliminary training in the observation of negative after-images.[1] Optimal fixation-times were determined for each *O* and were found to range from 15 to 30 sec. The fixation-stimulus consisted of a red square, 2.5 cm. on a side, fixated at a distance of 1 M. To eliminate all view of surroundings and to abolish cues to distance, observation was through a tube placed as close as possible to the eye. Projection of the after-image was accomplished by removing the fixation screen behind which was the projection screen at either 2, 5, or 6 M. (from the eye). By using only enough illumination on the projection screens to see and measure the image and by placing screens with apertures between the *O* and the projection screens, practically all cues to distance and vision of surroundings were eliminated.

To test the influence, if any, of the diameter and length of tube several tubes were used having the following dimensions: 24.5 x 2 cm.; 25 x 2 cm.; 25 x 2.5 cm.; 25 x 3.5 cm.; 37.5 x 3 cm.; and 49.5 x 3.3 cm. Within the range of these dimensions size of tube did not affect the results.[2]

* Accepted for publication April 5, 1935.

[1] I wish here to acknowledge the aid of Miss Lois Davis who acted as experimenter during most of the observations reported in this study.

[2] The effect of size of aperture on both after-images and directly perceived stimuli has been pointed out by A. Noll, Versuche über Nachbilder, *Psychol. Forsch.*, 8, 1926, 25, and N. R. F. Maier, The illusion of size in pinhole vision, this JOURNAL, 41, 1929, 291-295. Maier ascribes it to effects of accommodative changes but the writer has shown that pupillary changes themselves may affect apparent size. See H. Helson, Demonstration of pupillary, accommodative, and consensual reflexes through changes in apparent size of a pinhole, *J. Gen. Psychol.*, 13, 1935, 186-188.

The projection screens consisted of gray cardboard tacked to wooden stands on tables and were made so large that even at 6 M. only a homogeneous, gray background appeared through the end of the tube. The images were measured by bringing a meter stick in from the side until it reached the farther side of the image, after which the finger was moved along the stick until the *O* reported that the side near the *E* had been reached. Judgments of apparent size were made before measurement in terms of the categories 'smaller,' 'equal,' 'slightly larger,' and 'very much larger' than the stimulus. Reports were also asked on other characteristics of the image, such as shape, saturation, clearness, and apparent distance.

Results. The results obtained by measurement and report on apparent size were strikingly different. As will be seen from Table I, the measured sizes tend to conform to Emmert's law although the ratios are slightly larger than they should be if the law held perfectly. From the results for measured size alone we would never have suspected that Emmert's law does not hold

TABLE I

SHOWING THE PROJECTION DISTANCE (D), THE AVERAGE SIZE OF NEGATIVE AFTER-IMAGE (I), THE RATIO OF IMAGE TO OBJECT (I/O), AND AVERAGE PERCENTAGE FOR EACH CATEGORY OF JUDGMENT AT EACH D

D	I	I/O	Smaller	Equal	Slightly larger	Larger	Very much larger
2	6.32	2.5:1	3	34	15	18	27
5	13.94	5.6:1	2	40	12	19	28
6	17.74	7.1:1	1	41	18	17	26
Av.			2	38	15	18	27

for apparent size as well. Adding the average percentages for all *O*s in the first three categories of judgment, we find that 55% of all judgments on apparent size indicate that the projected images were at best only slightly larger than the stimulus. Since two-thirds of the observations were made at distances at which the images should have appeared at least five or six times as large as the stimulus, if Emmert's law holds for apparent size, the majority of judgments under definitely larger is very significant.

These results are made even more significant if we leave out the percentages for one *O*, (*E.F.*), who was strongly myopic and markedly different in her reports from the other four *O*s. She was responsible for all of the judgments of 'very much larger' and for 64% of the 'larger' judgments, giving no 'smaller,' 'equal,' or 'slightly larger' judgments. Omitting her results, the percentage of 'very much larger' judgments drops to zero, the 'larger' judgments rise to 25% from 18, but the total percentages in the other three categories rises from 55 to 75%. Furthermore, the lack of any increase in the percentages for 'larger' with increasing distance of projec-

tion and the modal percentage at 'equal' attest the size-constancy of the negative after-image under our conditions of observation.

We are now ready to take up the discussion of a possible explanation of all the results.

Discussion. The protocols of the *O*s contain several important statements throwing light on the probable explanation of the size-constancy of the after-images. One *O* noted that when the hand and ruler appeared they seemed very much reduced in size. In 8% of the reports, *O*s mentioned either a growing or shrinking movement in the image. In general, the image was localized rather indefinitely at the end of the tube showing that distance cues and the surroundings were fairly well eliminated. Even the projection screen appeared merely as a gray ground filling the end of the tube. These facts enable us to point to accommodative changes as the chief factor in our explanation, with other factors playing some part also.

The hand and ruler appeared smaller because, being out in space they necessarily subtended smaller visual angles and without the cues to distance and surroundings were seen as half their normal size. The after-image, due to a process on the retina which remains more or less constant, did not appear larger because with the surroundings cut off and no cues to distance, fixation was maintained on the end of the tube with a consequent reduction in apparent size. The strained accommodation also contributed to the diminished size of the hand and ruler.

That changes in apparent size can be effected by increased accommodation is a well-known fact[3] and can easily be demonstrated by fixating an object at about 3 M. and then fixating one at 10 cm., keeping the distant object in view. With near fixation the distant object shrinks to a remarkable degree, depending upon conditions of observation. *The change from minimum to maximum accommodation may result in a reduction of apparent size to one-fifth of the size given in normal vision.* In our experiments the apparent increase in size should have been of the order of a five- or six-fold increase, hence the size-constancy could well have been due to a reduction to one-fifth through accommodative changes alone. Since there were no cues to distance and the after-image was localized at the end of the tube only 25 cm. from the eye, accommodation was maintained when the *O* was supposedly 'projecting' the after-image at various distances. Constant accommodation plus constant retinal image gave constant apparent size.

Until the full effect of accommodative changes upon apparent size have been investigated it is useless to postulate 'inner forces' and other highly problematical processes both in direct vision, after-images, and in all cases involving apparent size. It seems very probable, for example, that some of the anomalies in apparent size reported by Noll and others,[4] can be explained in terms of accommodative changes.

The reports of *E.F.* who perceived the after-images as much larger than the other

[3] Helmholtz has made much of it and also J. von Kries. See H. von Helmholtz, *Treatise on Physiological Optics*, Eng. trans., edited by J. P. C. Southall, 1924, vol. 1, 127-8 and vol. 3, 389. The reduction to one-fifth original size is based on the work reported in the article by the writer cited in footnote 2.

[4] Noll, *op. cit.*

Os deserves special consideration. This *O*, being strongly myopic, was never able to achieve the relaxed accommodation characteristic of normal *Os*. She was never able to get full correction for her myopia and saw objects at almost all distances as more or less blurred. Her cues to apparent distance may well have been the clearness of the perceived object. By this is meant not merely the psychological clearness but rather the larger unclear portion of images which are out of focus. If one looks through a strongly convergent lens of from 10 to 20 diopters at a distant object beyond the principal focus of the lens, one will see two more or less distinct images: a central, fairly clear portion 'smaller than the object,' and an outer, hazy portion much larger than the central portion. The myopic *O*, unable to choose between the two portions based her judgments upon the unclear, hazy portion of what she saw. Her protocols bear this assumption out as she continually reported unclear, hazy images. Psychologically a more distant object is more blurred for this *O* since it falls outside the range of her accommodation and in this sense, too, the unclear images were judged as larger. We have no way of knowing whether the increase in apparent size which she reported follows Emmert's law or was an increase unrelated to the distance of projection. It is significant that her *measured* sizes are not larger, on the average, than the other *Os*—another fact pointing to the independent variability of measured and apparent size of the after-image.

Finally, we must point out that we do not regard accommodation as the sole factor responsible for the size-constancy observed under tube observation. The influence of surroundings and background are also of importance. With no surroundings or background to furnish a basis for 'comparison' the after-image tends to maintain its original size. By this we do not mean to imply that one consciously compares image and surroundings or background. Rather, the image 'covers' an area having a certain apparent size. The apparent size of the image depends upon the apparent area covered. When, due to accommodation, tube vision, etc., the image appears to cover no greater area with greater distance of projection, it tends to maintain its original size.

It may be objected that our *Os* were not really 'projecting' when they localized the images at the end of the tube instead of 'out there' on the gray screen. A moment's consideration will show that 'projecting' can only mean a certain state of affairs in the eye followed by certain changes in the central nervous system. If the image on the retina is fixed in size as it is in the negative after-image, then the difference between projecting to the end of the tube and beyond the end of the tube must be sought in accessory visual mechanisms and the surrounding visual field. The same argument applies to cases involving direct vision where apparent size may remain constant while the retinal image grows larger or smaller. Apparent distance and apparent size are closely linked with each other and both with accommodative and other changes. In this study the variable factor was the accommodation, if we neglect pupillary changes,[5] and to this we must look for the chief explanation of our findings.

[5] The influence of pupillary changes on apparent size through their effect upon the brightness of the image has been discussed by the writer above, *op. cit.* Since pupillary changes must have been of minor importance under our conditions we are safe in neglecting them.

SUMMARY

Measured and apparent size have been found to vary independently when surroundings and cues to accommodation are obliterated. Measured size of after-images varied directly with the distance of projection in conformity with Emmert's law. Apparent size, in the majority of reports for all except a myopic subject, remained practically constant regardless of distance of background of projection. Until the full effect of accommodation upon apparent size has been investigated and ruled out it is unnecessary to have recourse to assumptions involving purely hypothetical and unknown processes. The part played by 'covering' and clearness of the image in determining apparent size have also been stressed.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

X. Factors Determining the Affective Values of Color Combinations

By Elysbeth C. Allen and J. P. Guilford

In a previous study,[1] the senior author proposed a quantitative law of affective combination by which the affective value of a combination of two colors can be predicted from the affective values of the two components. This law was later found to apply to combinations of odors as well as colors.[2] In both cases, however, the predictions were not perfect, indicating that there are other factors determining the value of the combination. A search of the literature on color harmony finds many suggestions to account for 'good' and 'poor' color combinations. In fact this literature presents a veritable mine of hypotheses which are capable of direct scientific verification or refutation. Only a few of them will be tested in this study.

Many writers on the subject of color combinations have been concerned with such factors as differences in hue, tint, and chroma of the two colors, in one way or another.[3] Cohn[4] was one of the first to urge that contrasting hues and tints make the most agreeable combinations; he presented experimental results to support his contention. Other investigators, particularly Baker[5] and Metcalf,[6] have obtained results which contradict this general rule. Yet many 'authorities' who have written on the subject stress the importance of using complementaries or near complementaries in getting artistic effects. As a second choice, hues that are very close together on the color circle rather than those with a medium amount of difference are recommended. Neighboring hues are said to require a strong brightness-difference to be pleasant, and contrasting hues require a small brightness-difference.[7] Medium differences in hue are said to be worse if there is a brightness-difference.[8] There are other rules of good color combinations, but our data seem inadequate to the

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[1] J. P. Guilford, The prediction of affective values, this Journal, 43, 1931, 469-478.

[2] W. Spence and J. P. Guilford, The affective value of combinations of odors, this Journal, 45, 1933, 495-501.

[3] For a bibliography of the work on color preferences see A. R. Chandler, A bibliography of experimental aesthetics, 1865-1932, *Ohio State Univ. Stud., Bur. Educ. Res. Mimeo.*, 1933 (no. 1), 1-25. For a summary and critical discussion, see J. G. Beebe-Center, *The Psychology of Pleasantness and Unpleasantness*, 1932, 1-427.

[4] J. Cohn, Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten, und ihrer Combinationen, *Phil. Stud.*, 10, 1894, 562-602.

[5] E. S. Baker, Experiments on the aesthetics of light and color: I. On combinations of two colors, *Univ. of Toronto Stud., Psychol. Ser.*, 1, 1900, 201-249; II. Spectrally pure colors in binary combinations, *ibid.*, 2, 1907, 27-43.

[6] J. T. Metcalf, The pleasantness of brightness combinations, this Journal, 38, 1927, 607-623. This study deals exclusively with colorless stimuli.

[7] M. E. Chevreul, *The Principles of Harmony and Contrast of Colours*, 1890, 69 ff. Also W. von Bezold, *The Theory of Color*, 1896, 175 ff.

[8] Von Bezold, *op. cit.*, 185.

task of testing them. Our data can be used to indicate validity of the rules that have been listed in this paragraph only.

Method and Procedure

Selection of the color combinations. Forty-five pairs of colors were selected from the 90 colors of the Milton-Bradley series in the following manner. The 90 colors were given some preliminary ratings for hue, tint, and chroma, using the Munsell *Book of Color.* Eight pairs were chosen in which the two colors were practically equal for tint and for chroma but the hue differed from 2 to 9 steps on the Munsell scale. Six pairs were chosen with hue and tint constant but the differences in chroma varied 3 to 9 points on the scale. Eight pairs had hue and chroma relatively constant but tints differing from 2 to 5 steps. Six pairs had almost identical hues but differences in both tint and chroma. Six pairs were almost equal in tint but varied in both hue and chroma. Five more were equated for chroma with differences in hue and tint. The remaining 6 varied simultaneously in all three attributes of color, making a total of 45 color-pairs. The result was 18 pairs differing in hue by less than one step, the remaining 27 differing from 1 to 9.6 steps, 10 steps difference being exactly complementary. There were 19 pairs with less than one step of difference in tint, the remaining 26 differing from 1 to 5.3 steps. There were 20 pairs with less than two steps difference in chroma the remaining ones differing from 2 to 9.5 units. After the judgments for affective value had all been obtained, each *O* was asked to rate the colors according to the Munsell scales on two different days. The means of the 20 ratings were used as the final evaluations of hue, tint, and chroma of the colors used.

The colored papers were cut into rectangles 2 in. x 4 in. and the pairs were glued adjacent to each other in 4 in. squares on photo-mount cards having a neutral gray surface which rated exactly 5.0 points on the Munsell scale. In order to find the affective values of the single colors, they were cut into 4 in. squares and glued similarly on the neutral gray backgrounds. Both the single colors, numbering 40, and the combinations were presented to *O* for his judgments within the same hour of observation.

Procedure. Ten *O*s including 5 men and 5 women, 2 of them being instructors in psychology, 4 graduate students, and 4 undergraduates, served in the experiments. None had had any technical training in the fine arts. Each *O* judged the colors and the combinations 5 different times on 5 different days, using a rating scale of the numerical type, with values ranging from 1 to 9, and with an indifference point at 5. The stimuli were presented under artificial 'daylight' illumination for 5 sec., against a large neutral gray background. *O,* having his head fixed in a head-rest, looked into a small chamber whose walls were of the same gray material as that upon which the colors were glued. The stimuli were exposed in a small window at the rear. The order of presentation of stimuli was arranged so as to avoid adaptation and contrast effects. From the experiments we have the affective value of the 45 combinations as based upon the means of 5 judgments for every *O,* and the pooled ratings for 5 men and 5 women. We also have similar ratings for the affective values of the components of all the pairs, and the evaluations of hue, tint, and chroma for all the color components.

RESULTS

The law of affective combination. We first present Table I to show how well the affective value (AV) of a combination can be predicted from the affective values of the separate components. Linear functions were assumed because that is what were found in the previous studies. The multiple coefficients of correlation appear in column (1) and the partial standard regression coefficients (Beta coefficients) for the more pleasing component (P) appear in column (2), and the Beta coefficients for the less pleasing component (U) appear in column (3), for the *Os* separately and for the pools of judgments of women and men. On the whole, the predictions can be made more accurately for the women than for the men. The coefficients of multiple correlation (R) are smaller than those previously found when predicting from P or U, but all the multiple coefficients, save one, are very

TABLE I

COEFFICIENTS OF MULTIPLE CORRELATION AND STANDARD REGRESSION COEFFICIENTS FOR THE PREDICTION OF AFFECTIVE VALUES OF COLOR COMBINATIONS FROM THE VALUES OF THE COMPONENTS AND FROM THE DIFFERENCES IN TINT AND CHROMA

		(1)	(2)	(3)	(4)	(5)	(6)
	O	$R_{AV.PU}$	$\beta_{AVP.U}$	$\beta_{AVU.P}$	$R_{AV.TC}$	$\beta_{AVT.C}$	$\beta_{AVC.T}$
	A	.47	.20	.31	.38	.29	−.21
	B	.72	.12	.65	.26	.06	−.24
Women	*C*	.61	.22	.44	.32	−.22	−.27
	D	.70	−.11	.75	.10	.11	−.14
	E	.60	−.02	.61	.49	.45	−.13
	Mean	.64	.09	.57	.28	.21	−.17
	G	.15	.04	.13	.62	.61	−.07
	H	.70	.57	.22	.38	.38	−.06
Men	*I*	.55	.09	.52	.68	.68	.05
	J	.69	.21	.55	.53	.52	−.07
	K	.65	.28	.50	.29	−.30	−.04
	Mean	.58	.38	.25	.66	.65	−.04

significant.[9] The Beta coefficients give a fair idea of the relative importance of the two variables P and U in predicting the AV of the combination. For the women the less pleasing component, U, is decidedly the stronger factor but for the men there is disagreement as to which is the stronger factor.

It has been suggested before that perhaps the distinction between P and U variables is a rather artificial one.[10] A different type of distinction was tried with these data. We classified the component colors as stronger (S) or weaker (W), according as their affective values were farther from the indifference point or nearer to it. That is possible when a rating scale method is used and the indifference point has been established. As it happened with our particular selection of colors, however, the S-W distinction almost coincided with the P-U distinction because a majority of the single colors are above the indifference point, that is pleasant rather than

[9] Any R in this problem that is greater than 0.363 would be regarded as significant, and any R greater than 0.442 very significant, according to R. A. Fisher's table of significance as presented by H. R. Wallace and G. W. Snedecor, Correlation and machine calculation, *Iowa State Col. Pub.*, 1931, 62.

[10] Guilford, *op. cit.*, this JOURNAL, 43, 1931, 476.

unpleasant, so that in most cases a color that is P is also S, and one that is U is usually W. Only in a few cases was the less pleasing color farther from the indifference point and hence stronger than was the more pleasing one. As a matter of interest, we computed the multiple coefficients of correlation predicting the AV of the combination from S and W. There were small changes in R for different individual *O*s, but the Rs for the pooled ratings were almost identical with those in Table I, being 0.640 for the women and 0.589 for the men. Nothing is apparently gained from the practical standpoint of accurate prediction by using the S-W distinction, but logically it would seem to be much more significant than the P-U distinction. If our AVs for the components had been distributed equidistant above and below the indifference point, an increase in the Rs might have resulted.

Influence of differences in hue, tint, and chroma. Since earlier writers have urged that both small and large differences in hue are favorable for pleasing combinations, before attempting to correlate those differences with the AV of the combination we have attempted to see whether the supposed non-linear relationship holds. On account of the very wide scatter of the data when AV is plotted against difference

TABLE II

RELATION OF THE AFFECTIVE VALUES TO DIFFERENCES IN HUE

	Difference				
	0–.9	1–1.9	2–4.9	5–7.9	8–9.9
Women	.733	−.470	−1.74	−.072	.270
Men	−.062	.055	− .125	−.021	.192
N	10	10	8	11	6

in hue, very little notion could be gained concerning the type of relationship. We therefore eliminated the influence of other known factors in the following manner. We obtained regression equations for predicting AV from the two components, and from differences in tint and chroma. The Beta regression coefficients may be found in Table IV. We predicted the AVs of the combinations from the 4 variables, found the discrepancies between the predicted AVs and the observed AVs, and used these discrepancies to find the relationship between AV and differences in hue. Table II shows the mean discrepancies for various degrees of difference in hue. A negative average in Table II means that the effect of that particular difference in hue is to lower the AV of the combination and a positive mean indicates an enhancing effect.

The limited data of Table II are merely suggestive that there is some truth in the hypothesis that both very similar and contrasting pairs are pleasant. The women's results suggest this much more strongly than do the men's. Certainly, the data fail to show any continuous linear relationship between differences in hue and the AV of the combinations, and since the type of functional relationship is as yet unknown, no correlation between these two variables will be attempted.

When the AVs of the combinations are plotted against the differences in tint, the scatter diagram is apparently a linear distribution with a positive slope. The scatter diagram for the AVs plotted against the differences in chroma is likewise linear with a very slight negative slope. These facts being true, we felt justified in correlating the AV of the combinations with difference in tint (T) and with difference in chroma (C) and in finding a multiple R and the Beta coefficients. These results are given in Table I, columns (4), (5), and (6). There is little doubt from the results that the men are decidedly more responsive to these two

variables than are the women. The two multiple Rs are 0.66 and 0.28, respectively. With the exception of two *Os*, the greater the difference in tint the more pleasing the combination. All the *Os*, with one very doubtful exception, are slightly inclined to prefer their two components near together for chroma rather than far apart. The women are almost as responsive to homogeneity of chroma as they are to difference in tint, although not very responsive to either factor. Men are decidedly responsive to contrasting tints, but are practically unresponsive to the factor of homogeneous chroma. It is not to be overlooked that the responsiveness to either of these factors is contingent upon a concomitant discrepancy in hue, as some writers have pointed out. If a difference in hue were held constant the influences of T and C might be significantly enhanced. As it is, the men as a group are more influenced by differences in tint and in chroma than they are by the AVs of the two components. The two total Rs are 0.66 and 0.58, respectively.

We can also predict the AV of the combinations from all 4 independent factors, S, W, T, and C, simultaneously. The results appear in Table III. Only the two

TABLE III

MULTIPLE CORRELATIONS AND BETA COEFFICIENTS FOR PREDICTIONS OF AFFECTIVE VALUE FROM S, W, T, AND C

	$R_{AV.SWTC}$	$\bar{R}_{AV.SWTC}$	$\bar{R}^2_{AV.SWTC}$	$\beta_{AVS.WTC}$	$\beta_{AVW.STC}$	$\beta_{AVT.SWC}$	$\beta_{AVC.SWT}$
Women	.707	.674	.454	.304	.492	.207	−.166
Men	.881	.868	.753	.516	.375	.649	−.039

pools of data are represented. $R_{AV.SWTC}$ is the raw multiple R, uncorrected for the number of variables. $\bar{R}_{AV.SWTC}$ is the corresponding R after the correction.[11] $\bar{R}^2_{AV.SWTC}$ is known as the 'coefficient of multiple determination' and it estimates the proportion of the determination of the dependent variable that may be attributed to the combined influence of the several independent variables. Thus the AVs of the combinations for these 5 women are determined by the factors S, W, T, and C to the extent of about 45%. The AVs of the men are determined about 75% by those same factors.[11] We shall have to look elsewhere for the remaining causal factors. A comparison of the Beta coefficients suggests that for women the weaker of the two components has the greatest potency in determining the AV of the combination, and the difference in chroma has the least effect. For the men the stronger component has more effect than the weaker, as we should rather expect, but stronger than either is the factor of difference in tint. Difference in chroma has practically no effect for the typical man in this group.

Influence of differences in hue and tint. What now of the rules that neighboring hues require a large difference in tint to be pleasant, whereas medium or large differences in tint are better with small brightness differences? The strong correlation between brightness difference and AV, especially in the case of the men's data, argues in favor of a large brightness difference, no matter what the difference in hue may be. To test the question further, we have fractionated our data into 3 sets, separating out the small, medium, and large differences in hue. Small differences were those less than 2 units of the Munsell scale; medium differences were greater than 1 but less than 8; large differences were greater than 6. There is thus some overlapping, but this is in order not to restrict the numbers of cases within each category too much. Within each one of the 3 groups the combinations were sub-

[11] M. Ezekiel, *Methods of Correlation Analysis*, 1930, 177.

divided into large and small differences in tint. Small differences were those less than 2 units on the Munsell scale, and large differences those greater than 2 units. Table IV shows the resulting differences in AVs under the different groupings.

The numbers of cases are very small indeed, and we have accordingly used (N-2) instead of N in computing the standard deviations of the means when N is 10 or above and we used (N-3) when N was less than 10.[12] This gives a conservative estimate of the sigma of a difference and also of the critical ratios. In spite of the limited data the critical ratios for the men at least are significant. The negative differences indicate that the rules of the 'authorities' are wrong. In every one of the 3 sets of data, a large difference in tint is on the whole preferred to a small difference. Regardless of what the difference in hue is, then, the same relationship of brightness-differences to affective value holds. The law is upheld almost with statistical assurance in the case of the men's data and not for the women's data, just as the correlation coefficients would lead us to expect. We can be reasonably sure that the lower multiple Rs for the women in column (4) of Table I are

TABLE IV

DIFFERENCES IN MEAN AFFECTIVE VALUES OF COMBINATIONS HAVING LARGE OR SMALL DIFFERENCES IN TINT WHEN THE SIMULTANEOUS DIFFERENCES IN HUE ARE LARGE, MEDIUM, AND SMALL

	Contrasting hues (H>6)			Medium hue diff. (1<H<8)			Neighboring hues (H>2)		
	Women	Men	N	Women	Men	N	Women	Men	N
Tints nearly the same	4.76	4.41	8	4.17	3.83	6	4.79	4.47	10
Tints differing	5.06	6.00	6	4.54	5.18	8	5.55	5.32	12
Diff. in AV	−.30	−1.59		−.37	−1.35		.86	.85	
CR_d	−.45	−3.70		−.36	−2.50		1.73	2.80	

not due to any contaminating effect of differences in hue. We say "reasonably sure" here because what little evidence there is in Table IV is in favor of large brightness-differences as opposed to small, and it is consistently so for all degrees of difference in hue.

SUMMARY AND CONCLUSIONS

Forty-five color combinations were carefully selected as to differences in hue, tint and chroma. Ten *Os*, 5 men and 5 women, judged the affective values of the combinations using a rating scale method. The following conclusions are indicated by the results.

(1) The law of affective combination was fully verified; the affective value of a combination is highly dependent upon the affective values of the components.

(2) There is some evidence that either very small or very large differences in hue give more pleasing results than do medium differences. This tendency is much stronger for women than for men.

(3) The affective value of a combination is positively correlated with the amount of contrast in tint, regardless of whether the difference in hue is great, medium, or small. This correlation is much stronger for the men than for the women.

(4) There is a slight preference for combinations with small differences in chroma rather than large differences. This is a negligible factor in the case of the men.

[12] J. W. Dunlap and A. K. Kurtz, *Statistical Nomographs, Tables and Formulas*. 1932, 103.

TASTE DEFICIENCY OF JAPANESE AND OTHER RACES IN FORMOSA

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The present paper is a report of a gustatory test carried out in Formosa, upon a large number of Japanese, Formosans, and native savages, with a chemical substance similar in its constitution to the compound first used by A. L. Fox and, subsequently, by many American workers.[1] The substance, para-ethoxy-phenyl-thio-carbamide, was derived and purified from para-phenetidin by Professor Nozoye of our University. The melting point of it was 172°-172.5°.

The white crystals of the compound were carefully ground to the finest powder in a mortar. Special cards were prepared with blanks to be filled in with regard to the blood-relationship, age, sex, and taste-reaction of the *Os*. The categories for the taste-reaction were *tasteless, slightly bitter, rather bitter, very bitter, sweet, sour, salty,* and *astringent*. The last term was adopted because of its familiarity among laymen. A single card could be used for an entire family, for it had blank columns for several persons, and there was space provided where each individual could note his taste-reaction. The card was put into an envelope with a sheet of directions and a small quantity of the stimulus-compound in the form of dry crystals, and the envelopes were taken home by each of the school children. The taste-reactions of the natives were examined by the writer himself with a great deal of help, especially in linguistic matters, from the police staff in charge at the locality.

INDIVIDUAL DIFFERENCES

Table I annexed tells its own story. It confirms the existence of very considerable individual differences in the reactions to the same compound. No criterion for the degree of bitterness was established in advance; the subdivisions of the bitter class as shown in the table are based entirely upon the subjective judgment of the individual tested. Consequently the individuals in the three sub-groups of *bitter* may in some measure be interchangeable with one another. Nevertheless there is no doubt from the remarks put in the cards that a great deal of variety in degree of bitterness was experienced by different *Os*. That the compound gives a more or less bitter sensation to the majority of people is clearly seen from Table I.

On the other hand, for a number of persons this identical stimulus, which was almost unbearably bitter for some, aroused no taste at all. The writer himself belongs to the 'tasteless' group and is never able to experience taste either from

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[1] Anon., *Science,* 73, 1931, 14; A. F. Blakeslee and M. R. Salmon, Odour and taste blindness, *Eugen. News,* 16, 1931, 105-109; L. H. Snyder, Inherited taste deficiency, *Science,* 74, 1931, 151-152; Blakeslee, Genetics of sensory thresholds: Taste for phenyl-thio-carbamide, *Proc. Nat. Acad. Sci.,* 18, 1932, 120-130; A. L. Fox, The relationship between chemical constitution and taste, *ibid.,* 115-120; S. W. Fernberger, A preliminary study of taste deficiency, this JOURNAL, 44, 1932, 322-326; P. Levine and A. S. Anderson, Observations on taste blindness, *Science,* 75, 1932, 497-498; Blakeslee and Fox, Our different taste worlds, *J. Hered.,* 23, 1932, 97-110.

the dry crystals or from a saturated solution, or even from a warm pure alcoholic solution.

Other reactions were often reported, especially sweetness. Requested to do so, most of the *O*s indicated the substance which was supposed by them to have a taste similar to the taste of the stimulus. Some *O*s stated that the stimulus was "as sweet as sugar or candy," some "as sour as Japanese pickled plums," some "as salty as table salt," and the others "astringent like an unripe tomato." Many other common sapid foods and drugs were mentioned. The reality of these differences is, therefore,

TABLE I

DISTRIBUTION OF THE TASTE REACTION TO THE COMPOUND

Reaction Category	Japanese		Formosans		Natives	
	No.	Percent	No.	Percent	No.	Percent
Tasteless	1264	14.32±.25	422	7.11±.23	31	1.77±.21
Slightly bitter	662	7.50±.19	520	8.76±.25	217	12.36±.53
Rather bitter	1005	11.39±.23	975	16.43±.32	218	12.41±.53
Very bitter	5648	64.01±.34	3814	64.28±.42	1229	69.99±.74
Sweet	104	1.18±.08	80	1.35±.10	28	1.59±.20
Sour	46	0.52±.05	31	0.52±.06	3	0.17±.06
Salty	45	0.51±.05	43	0.72±.07	26	1.48±.19
Astringent	50	0.57±.05	48	0.81±.08	4	0.23±.08
Total	8824	100.00	5933	100.00	1756	100.00

beyond question. In the case of the natives, since the test was carried on individually and orally, the *O*, when his answer was other than *bitter*, was required to mention the objects supposed to have a similar taste or oral feeling.

The age of the *O*s ranged from 2 to 90 (though the number of *O*s in the extremes were few), and varieties of reactions were found throughout the whole range.

It is plain from the data given in Table I, that there are great individual differences of reaction to this organic compound, in respect both of intensity and of quality.

TABLE II

DISTRIBUTION IN PERCENTAGES OF THE TASTE REACTION: CONDENSED FORM OF TABLE I

Reaction	Japanese	Formosans	Natives
Tasteless	14.32±.25	7.11±.23	1.77±.21
Bitter	82.90±.27	89.47±.27	94.76±.36
Sweet and others	2.78±.12	3.40±.16	3.47±.29

RACIAL DIFFERENCES

Table I shows that individual difference is common in all the races tested. Each race afforded some cases in every category. A closer inspection of the data, however, shows that there are great differences between the races. By bringing together the three sub-classes of bitterness into one and by grouping the small frequencies, we obtain Table II, which is more convenient for comparing the races.

Table II shows at least two interesting differences between races. (1) The percentage for the persons who fall in the class *tasteless* progressively diminishes as we pass from the Japanese to the native group. (2) The proportions for the class *bitter* increase in the same order. (3) The last item, 'sweet and others,' increases in fre-

quency, from left to right. Table III shows the statistical significance of these differences. From it we perceive that the racial differences in the percentages for the last item, 'sweet and others,' are all too small in relation to their probable errors to ensure significance.

For the other two categories, *tasteless* and *bitter*, there is no question as to the existence of significant differences in the relative frequencies between the races.

TABLE III

DIFFERENCES IN PERCENTAGE BETWEEN DIFFERENT RACES

Reaction	Japanese and Formosans			Japanese and Natives			Formosans and Natives		
	Diff.	P.E.$_{diff.}$	$\frac{\text{Diff.}}{\text{P.E.}_{diff.}}$	Diff.	P.E.$_{diff.}$	$\frac{\text{Diff.}}{\text{P.E.}_{diff.}}$	Diff.	P.E.$_{diff.}$	$\frac{\text{Diff.}}{\text{P.E.}_{diff.}}$
Tasteless	7.21	.34	21.21	12.55	.33	38.03	5.34	.31	17.23
Bitter	6.57	.38	17.29	11.86	.45	26.36	5.29	.45	11.76
Sweet and others	0.62	.20	3.10	0.69	.31	2.23	0.07	.33	.21

The frequency for *tasteless* among the Japanese is about twice as large as among the Formosans; and the frequency among the natives is only one eighth of that of the Japanese and one quarter of that of the Formosans. Table IV shows the frequency of non-tasters found among three native tribes taken separately. Here differences are apparent between the different tribes.

It is obvious that there exists a striking difference in the frequency of occurrence

TABLE IV

FREQUENCIES OF NON-TASTERS AMONG NATIVE TRIBES

Tribes	No. of non-tasters	Total cases tested	Percentages
Atyal	26	581	4.48±0.58
Amis	4	718	0.56±0.19
Tsou	1	457	0.22±0.15
Total	31	1756	1.77±0.21

of non-tasters between the different races. A unique support for this finding is the study of Levine and Anderson.[2]

CONDITIONS SUPPOSED TO AFFECT RACIAL DIFFERENCES

It has been suggested that these racial differences might prove to be functions of region of nativity, or of sex, or of age. These three possibilities have been considered in Tables V to VII.

TABLE V

LOCAL DISTRIBUTION OF THE NON-TASTERS: FORMOSANS

City	No.	Percent
Taihoku	220	6.85±.30
Tainan	109	7.77±.48
Shōka	93	7.06±.48
Total	422	7.11±.23

[2] Levine and Anderson, *op. cit.*

Local differences. Since the Japanese are rather recent immigrants to Formosa and have come from various places in Japan, they are not appropriate *Os* with whom to study local influences. The Formosans, on the contrary, are satisfactory in this respect.

TABLE VI

SEX-DIFFERENCES IN PERCENTAGES OF NON-TASTERS

Race	Men	Women	Diff.	$\frac{\text{Diff.}}{\text{P.E.}_{\text{diff.}}}$	Chances in 1000 that difference is significant
Japanese	14.19±.37	14.44±.34	0.25±.50	.50	266
Formosans	7.50±.36	6.83±.29	0.67±.46	1.46	678
Atyal	5.90±.91	2.90±.68	3.00±1.14	2.63	925

Table V shows the distribution of the non-tasters of the Formosan *Os* taken from three different cities and their vicinities—the cities of Taihoku situated in the north, Shōka in the center, and Tainan in the south of Formosa. It is evident that the differences are not statistically significant, since the largest is only 1.61 times its probable error.

TABLE VII

PERCENTAGES OF NON-TASTERS IN EVERY TWO YEARS OF AGE: JAPANESE

Age	Men	Women	Difference	
			Diff. (%)	$\frac{\text{Diff.}}{\text{P.E.}_{\text{diff.}}}$
2	12.5±7.88*	18.2±7.84*	(−5.7) —	—
4	11.1±2.13	15.0±2.33	−3.9±3.16	1.23
6	12.6±1.56	13.4±1.57	−0.8 —	—
8	12.4±1.36	13.7±1.36	−1.3 —	—
10	12.5±1.20	10.5±1.07	+2.0±1.61	1.24
12	11.5±0.94	13.9±1.04	−2.4±1.40	1.71
14	15.5±1.32	17.0±1.17	−1.5±1.77	0.85
16	18.5±1.93	15.1±1.09	+3.4±2.22	1.53
18	14.9±1.93	15.9±1.42	−1.0 —	—
20	9.9±1.83	16.5±1.89	−6.6±2.61	2.53
22	17.5±2.34	14.1±2.02	+3.4±3.09	1.10
24	12.1±2.21	14.7±2.90	−2.6±3.65	0.71
26	15.4±2.76	28.6±4.35	−13.2±5.15	2.56
28	5.9±2.72*	0 — *	— —	—
30	24.4±4.52	16.3±3.80	+8.1±5.91	1.37
32	6.7±3.08*	8.8±1.80	(−2.1) —	—
34	14.3±4.46*	21.7±2.26	(−7.4) —	—
36	8.2±2.64*	15.9±1.89	(−7.7) —	—
38	18.5±2.16	13.2±1.63	+5.3±2.71	1.96
40	14.4±2.01	8.7±1.40	+5.7±2.45	2.33
42	17.1±1.95	17.0±2.18	+0.1 —	—
44	18.1±1.89	12.3±2.15	+5.8±2.87	2.02
46	15.3±2.02	7.1±1.88*	(+8.2) —	—
48	12.2±1.82	17.0±2.53	−4.8±3.12	1.54
50	19.2±2.57	16.2±3.01	+3.0±3.96	0.76
52	13.6±2.46	18.6±4.00	−5.0±4.70	1.06
54	17.2±3.18	18.8±4.66	−1.6 —	—
56	8.1±3.02*	11.1±4.08*	(−3.0) —	—
58	14.0±3.57*	8.3±5.37*	(+5.7) —	—

* Insignificant percentage.

Sex differences. Table VI gives the data for sex differences. None of the differences between the sexes is large enough to claim statistical significance.

It is possible, however, that there might appear significant sex differences at particular ages. In Table VII the data for sex differences have been fractionated by two-year age-groups. There is no significant trend in these data either.

Age differences. Table VII can also be studied with respect to age. If certain instances, where the number of cases is too small to render the datum significant, are neglected, then no significant age differences appear.

SUMMARY

(1) Para-ethoxy-phenyl-thio-carbamide in dry crystals was tasted by a large number of persons of different races in Formosa, Japan. The resultant reactions of every *O* to the compound were reported either orally or entered in writing on special blank forms.

(2) In every race the reactions showed great individual differences both as to quality and as to intensity.

(3) In every race the frequency percentage of the *bitter* reaction was the highest, followed by the *tasteless* reaction, and then by the 'sweet and others.'

(4) Large differences in percentages for *bitter* and *tasteless* were found in connection with differences in race. The *tasteless* occurred most frequently among the Japanese, with a frequency percentage of 14.32%, while the rate for the Formosans was 7.11% and that of the natives 1.77%. The three native tribes also showed significant differences from one another.

(5) The percentages of tastelessness seem to have no connection with differences of habitat, of sex, or of age of the *O*s in the present sample.

TWO DETERMINANTS OF THE EFFECT OF PRIMACY

By GERTRUDE RAFFEL, Radcliffe College

Robinson and Brown have described several experiments which deal with the effect of serial position in learning.[1] In almost every case, including the experiment which they performed, the first members of a series are learned better than the middle portion, and are often learned more readily than the end. In an experiment by Welch and Burnett,[2] however, there was no effect of priority: the data of 26 *O*s who learned series of eight units showed approximately the same scores for each of the first six items, an appreciable rise for the seventh and an extremely high score for the last. The absence in this case of the usual facilitatory effect of primacy has received two explanations: one from Welch and Burnett, and the other from Robinson and Brown.

Welch and Burnett instructed their *O*s to stop attending to any syllable as soon as the next appeared. This procedure obviated the possibility of repetition of the earlier members of a series; and no effect of primacy was present. They believe that this effect is due, when it occurs, to frequency of repetition of the first item or items during the learning period. Robinson and Brown have rejected this suggestion because they think it improbable that there can be rehearsal of the first units when the *O* reads each syllable aloud as it appears. They note that Welch and Burnett did not control the order of recall: as soon as the list had been shown, the *O*s were allowed to report the last or last two groups. They suggest that the shorter time between learning and recall may, therefore, have weighted the data unduly in favor of the end of the list.

There has been little direct experimental evidence as to the importance of the time between learning and recall; but the results of Smith[3] and of Anderson and Crosland[4] are closely related and seem opposed to the explanation offered by Robinson and Brown. Smith allowed recalls to be made in any order; the *O* usually reported the first two and then the last two nonsense syllables of a list, and then whatever else he could remember. These data indicate that the part of a series which has been learned best is reported first, whether or not it was most recently presented. Anderson and Crosland determined the effect of order of report for material presented tachistoscopically. After exposure of the stimulus card their *O*s were instructed to report from left to right, or from right to left, or beginning at the center and alternating. No observable difference in the effect of position appeared under these three conditions. The experimenters conclude that priority or primacy of report is a relatively unimportant matter.

In an experiment on retroactive inhibition, the author has investigated this question by comparing two methods of recall. In one method the *O* reported the

* Accepted for publication March 9, 1935.

[1] E. S. Robinson and M. A. Brown, Effect of serial position upon memorization, this JOURNAL, 37, 1926, 538-552.

[2] G. B. Welch and C. T. Burnett, Is primacy a factor in association-formation?, this JOURNAL, 35, 1924, 396-401.

[3] W. G. Smith, The place of repetition in memory, *Psychol. Rev.*, 3, 1896, 21-31.

[4] I. Anderson and H. R. Crosland, A method of measuring the effect of primacy of report in the range of attention experiment, this JOURNAL, 45, 1933, 701-713.

items of a series in any order as soon as the series had been shown; in the other he reported only one predetermined item from each series he learned. The time between exposure and report of that item was kept the same for all the positions in the list. The *O*s were not cautioned against rehearsal. The results of 4 *O*s for 20 series of five units are as follows:

	Position				
Time intervals	1	2	3	4	5
Constant	1.0	1.0	1.1	1.9	3.0
Uncontrolled	1.1	1.5	1.7	2.0	2.5

The maximum score was 3.0.

Primacy produces no positive effect with either method; but finality does. These results show that an uncontrolled time-interval between learning and report may exaggerate the advantage of the last member of a series, but that it does not alter the trend of position effects.

The present experiments investigate the effect of primacy by modifying the anticipation method, in which the effect is pronounced, until a method of learning and recall is found in which the effect has disappeared. The relation between changes in method and changes in the effects of position provide the ground from which we may infer certain conditions which favor the earlier members of the series.

PROCEDURE

The material consisted of series of 8 nonsense syllables, exposed for learning on a memory drum for 2 sec. each. The syllables were chosen from those which Glaze[5] gives as having poor association value. The *O* was instructed to read each syllable aloud as it appeared. Reproductions were oral. One attempt at recall was required for every presentation of a list. Robinson and Brown had found that the same tendencies in position effects were present whether the lists were partially or fully learned. In this experiment partial learning was used: learning was terminated when the *O* could report 3 or more syllables correctly in one recall. The data (Fig. 1) are presented in terms of the percentage of correct responses for each position when the criterion has been attained. The same *O*s were not used for all the methods. For Method I, 5 *O*s learned 26 lists; for Method II, 5 *O*s learned 33 lists; for Method III, 4 *O*s learned 28 lists; for Method IV, 6 *O*s learned 53 lists; and for Method V, 4 *O*s learned 29 lists.

Method I. This is the anticipation method and is similar to the procedure of Robinson and Brown. Its essential feature is that, after the first exposure of the list, each syllable serves as a signal to report the next. Two sec. before each exposure of a series *E* said "Now," and the *O* tried to report the first syllable; when the first syllable appeared he tried to report the second; the second was his cue to recall the third, and so on. He was instructed not to try to remember the series between exposures.

Method II. After each presentation of the list the memory drum continued to revolve. When the first blank space appeared, the *O* tried to report the first syllable; 2 sec. later, when the drum turned again, he tried to report the second; and so on. The time and the order of report are controlled with both of these procedures,

[5] J. A. Glaze, The association value of non-sense syllables, *Ped. Sem.*, 35, 1928, 255-269.

but the anticipation method places more emphasis on the sequence of the items and on their serial positions.

Method III. After each presentation the *O* tried to report the syllables in order. This differs from Method II in that the time of report is not controlled.

Method IV. After each presentation the *O* was allowed to report the syllables in any order, with unlimited time.

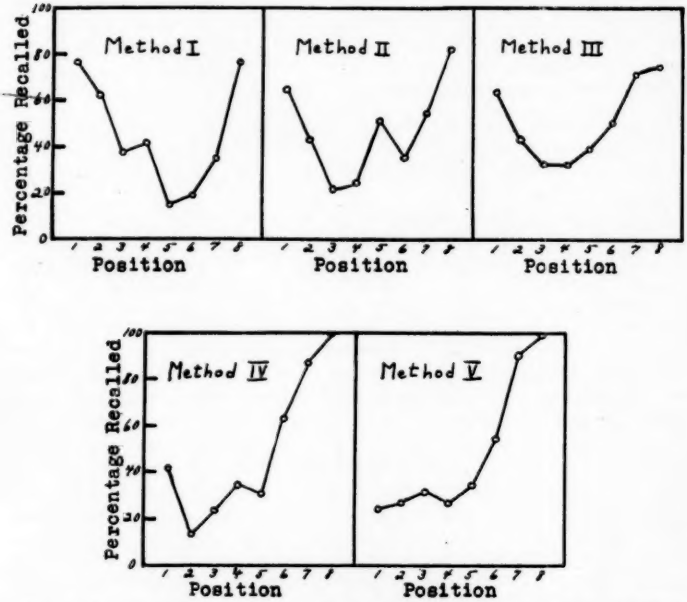

FIG. 1. COMPARISON OF POSITION EFFECTS IN A LEARNED SERIES UNDER FIVE METHODS OF LEARNING AND RECALL

Method V. The *O* was instructed to concentrate on each syllable as it appeared and not to repeat a syllable previously shown during the exposure of a later one. In other respects this procedure was the same as in Method IV.

RESULTS

The data are presented in Fig. 1.

The results with the anticipation method (I) are similar to those reported by Robinson and Brown: the primacy effect extends over more units than does the effect of finality. With Method II, however, the finality effect is more marked and extends over more units than does the primacy effect. In other words, decreasing emphasis on serial position causes a diminution in the advantage due to primacy.

Method III gives approximately the same curve for position effects as did Method II. Under these instructions the *O* would have fulfilled the criterion of learning if he had reported the sixth, seventh and eighth syllables as soon as the list was shown.

If the most recently presented syllables were the most effective because of their recency, these positions should have been recalled in preference to the first five. The fact that the data for Method III are the same as for Method II, when the interval between learning and recall was strictly controlled, is another indication that the time between learning and recall is not crucial with time intervals of this magnitude. Any disadvantage for the beginning of a list when the order of report is uncontrolled cannot, therefore, be assigned solely to the fact that recall of the end of the list is allowed to take place immediately after learning.

With Method IV the effect of primacy is slight, appearing only for the first syllable. The recency effect increases. The difference between this method and the last lies in the fact that the order of report was not controlled, a difference comparable to the difference between Methods I and II, since both Methods II and IV diminish the emphasis upon serial position of the items. The shift in the curves is also the same in both cases.

With Method V the primacy effect disappears completely. This result supports the contention of Welch and Burnett that there may be repetitions of the first part of a list during the learning of the latter part, so that the first position is more strongly impressed and therefore more accurately recalled than it would be without such repetitions.

Although there are no complete records of the number of presentations needed to reach the criterion of learning with the different methods employed, it was clear that Method I (anticipation) was much more difficult than the others, and that Methods IV and V offered the most favorable conditions. The instructions in these latter cases allowed the *O* to recall the material in the way most natural to him. Apparently when no restrictions are imposed upon recall, learning is most rapid, and the end of a list is learned best. The writer suggests that when rehearsals are allowed during learning, frequency of recall of the first item or items may cause the primacy effect; that, with instructions to recall a list in order, the *O* is set to learn the beginning and thus his learning shows an advantage for primacy; that under the most favorable and least restricted conditions of recall of nonsense material, there is a strong advantage for finality, but none for primacy. Such position effects could be predicted from the theory of retroactive inhibition, because learning has been interpolated after the beginning of a list, but not after the end.

SUMMARY

In this experiment the method of learning and recall of a series of nonsense syllables was varied, and the data show a primacy effect that is stronger than the recency effect under one set of conditions, no primacy effect under another, and two intermediate stages. Comparison of the different methods indicates that, when a uniform series is presented, instructions which permit repetition of initial syllables while later syllables are exposed favor the beginning of the list, as do instructions which require serial recall.

The diminution or disappearance of the primacy effect when the order of report is uncontrolled is not due to an uncontrolled time interval between learning and recall. It probably results from the set of the *O*, who is allowed to distribute his effort naturally under these conditions, but who concentrates on the beginning of the list when he is to be required to recall the syllables in the order of their presentation.

APPARATUS

ELECTRODES USED IN MEASURING ELECTRODERMAL RESPONSES

By D. Ulrich Greenwald, University of Iowa

It is well known that the perfect electrode for use in the measurement of the electrodermal response has not yet been developed. The major problem has been to minimize the change in polarization and resistance which takes place within the electrode when the measuring current flows through it. Electrodes are known to differ in this respect but there is very little material in the literature regarding their actual performance. For our own use we have investigated the potentials of polarization and the resistance changes in the Ruckmick-Patterson finger electrodes,[1] the simple zinc plate electrodes that have been used by Hathaway,[2] Ruckmick,[3] and Greenwald,[4] and the modified DuBois Reymond $ZnSo_4$, NaCl electrodes which have been used by numerous investigators.[5] These represent types which are advantageous from the standpoint of various applications and the data presented below should be generally useful.

The measurements were made with the customary experimental situation in mind. Inherent potentials and resistances were measured at the start and at the close of 30 min. intervals (with some exceptions), using a constant current such as is provided in most present-day apparatus. The value of this current was in all cases 0.09 mils., the maximum variation being 0.08%. Measurements of resistance were made only to the nearest 100Ω since this value represents 1/10 of the smallest electrodermal response usually reported. Potentials were measured on a galvanometer having a figure of merit of 3.0 μv. per mm. scale deflection. The resistance value obtained was checked by reversing the electrode leads and two values are reported wherever a difference was discovered. The effect of the direction of the measuring current with respect to the potential of the electrodes was also checked by repeated experiments with reversed leads. The temperature factor was no more closely regulated than in the usual experiments. All measures were taken in a room in which the temperature was thermostatically maintained at 75°F.

The Ruckmick-Patterson electrodes. The electrodes used by Ruckmick and Patterson are essentially a modification of the calomel cell used by chemists in electrometric

[1] C. A. Ruckmick and E. Patterson. A simple non-polarizing electrode, this Journal, 41, 1929, 120-121.

[2] S. R. Hathaway, A new psychogalvanic apparatus, *J. Appl. Psychol.*, 13, 1929, 632-638.

[3] C. A. Ruckmick, A new electrode for the Hathaway galvanic reflex apparatus, this Journal, 42, 1930, 106-107.

[4] D. U. Greenwald, An apparatus for measuring the electrodermal response, *ibid.*, 47, 1935, 682-685.

[5] Darrow has described one of the most recent modifications of this electrode which combines certain desirable features. There are a number of different designs to be found in the literature. Cf. C. W. Darrow, Relatively non-polarizing constant moisture, constant pressure, constant area electrode, *J. Gen. Psychol.*, 6, 1932, 476-478.

titrations. Information and references additional to the author's description may be obtained from Popoff.[6] The electrodes were prepared according to the authors' instructions and were connected by means of a salt bridge across the openings for *S*'s fingers. Measurements of potential and resistance were made as itemized in Table I. These values represent averages of three or more determinations.

It appeared from this table that following a period of settling after the electrodes were prepared their resistance value remained constant and at a fairly low level. The inherent polarization potential, however, tended to increase to a certain extent as the electrodes approached equilibrium. A constant current passed through them in the direction of the existing potential increased the value of this potential; the inverse relationship also obtained. Shorting the electrodes caused a slow loss of potential. The important observation, however, was that the resistance value remained constant within 100Ω during at least a half hour of current flowing in one direction.

TABLE I

NEW RUCKMICK-PATTERSON ELECTRODES

Conditions	μv.	Ω	Notes
Unsettled			
Settled 6½ hr.	21.0	400	
Shaken by jarring	27.0	400	
D.C. 30 min.	1.5	400	Opposing electrode potential
D.C. 30 min.	55.5	400	Reënforcing electrode potential
Settled 24 hr.	171.0	375	
D.C. 30 min.	225.0	375	Reënforcing electrode potential
*Shorted by galv. 45 min.	1.8	375	
D.C. 30 min.	0.0	375	Opposing electrode potential

* The coil resistance of the galvanometer was 20 Ω.

Measurement of these electrodes using a 60~current of the same value as the D.C. showed that they had no measurable resistance, or variation in resistance, on either the Hathaway or Greenwald apparatus during an hour of current flow. Five measurements revealed an average inherent potential of 48 μv. which value was maintained during the period of measurement of one hour.

The electrodes were unused for a week and then remeasured. Only a slight change in potential value was found but the resistance to D.C. had increased to approximately 800Ω, a value which was constant during an hour of current flow. After two weeks of disuse the resistance had increased to 1500Ω or 2000Ω depending upon the direction of the measuring current. The inherent potential value was about 1/3 of that in Table I. The resistance had begun to become unstable. The data on the changes which had occurred by the end of three weeks are given in Table II.

The characteristics of these electrodes, which were three weeks old, were the reverse of those in Table I. Their polarization potential, after current had passed for an hour, was only 1/15 of the potential set up by a 30-min. use of the current at the 24-hr. stage. On the other hand, their resistance had increased about ten times its 24-hr. value and exhibited considerable instability.

An attempt was made toward restoration of the former state by stirring, care being used not to disturb the bottom layer of mercury. The potential immediately increased

[6] S. Popoff, *Quantitative analysis*, 1927, 438-439, 450.

to twice the 'fresh' value and the resistance varied in relation to the direction of the measuring current. Allowing the mixture to settle for 24 hr. gave potential and resistance values which approximated those of the 'fresh' electrodes.

The designers of these electrodes recommended the use of 0.1 N. NaCl solution for making contact with *S*'s fingers. Due to evaporation and additions of more solution this normalcy is difficult to maintain. A saturated solution is easier both to prepare and maintain. We remade these electrodes using a saturated solution and found that so far as potential and resistance values were concerned the ones obtained were almost identical with those of the 0.1 N solution. Repeated experimentation failed to disclose any advantage of the designer's solution over the saturated solution from an *electrical* standpoint.

The question remains as to what condition of these electrodes introduces the least error. When freshly made the electrodes exhibit a constant resistance to a constant current but polarize to fairly high potentials. When old they polarize very little but show considerable variation in resistance. The greatest 'electrode' error is likely to occur in the case of an *S* with a low resistance. Consider a theoretical *S*'s resistance of 5000Ω in a constant current circuit. Suppose a potential of 90 v. is impressed upon a total resistance of one megohm which includes *S*'s resistance. The current in such a circuit would be 0.09 mils. as was the case in the above measurements. The potential across *S* would be 0.45 v. or 2000 times the highest polarization potential discovered in Table I. The error introduced by such a polarization potential must be very small. In such a circuit a resistance variation of 1300Ω as was found, however, in the aged electrodes (Table II) would cause an error of 26% in the case of our theoretical *S*. The argument is clearly for the fresh, or freshly stirred, electrodes which have settled until their resistance value has become constant. Further, this constancy of resistance can be checked easily by most electrodermal response apparatus if a salt bridge connection is used. There should be no objection to their use if this resistance remains constant during the course of an experiment to the smallest degree of resistance variation measurable with the particular electrodermal response apparatus used.

TABLE II

RUCKMICK-PATTERSON ELECTRODES THREE WEEKS OLD

Conditions	μv.	Ω	Notes
First measurement	1.0	3700	
Leads reversed	1.0	3280	
D.C. 30 min.	1.5	3000	Reënforcing electrode potential
D.C. 60 min.	15.0	2800	Reënforcing electrode potential
Shorted by galv. 30 min.	1.2	2450	
Stirred	404.0	400	
Leads reversed	404.0	600	
Settled 30 min.	98.0	200	
Leads reversed	98.0	300	
D.C. 30 min.	270.0	200	Reënforcing electrode potential
Settled 24 hr.	192.0	300	
D.C. 30 min.	78.0	300	Opposing electrode potential
Shorted by galv. 30 min.	24.0	300	
D.C. 30 min.	260.0	300	Reënforcing electrode potential
Shorted by galv. 30 min.	33.0	300	

Zinc plate electrodes. Measurement of the electrical characteristics of zinc plate electrodes is rendered difficult by the conditions one must attempt to reproduce. The conductor between the cells in the sweat glands, which are responsible for the electrodermal response, and the electrode is the perspiration of the body under the area which they cover. A 'perspiration conductance' must be used to connect the two plates if we wish to duplicate the conditions of their use in an experiment. Human perspiration is a complex solution containing in the 'normal' individual approximately 98% water with dissolved NaCl, phosphates, sulfates, lactates, a small amount of urea, the compounds of epithelial cells, as well as other solids in small amounts. The salt which comprises the greater part of the 1.2% of inorganic matter normally present is, however, NaCl. We roughly duplicated experimental conditions in the use of plate electrodes in the following way. One gram of NaCl

TABLE III

ZINC PLATE ELECTRODES

Conditions	μv.	Ω	Notes
First measurement	450.0	0	
D.C. 15 min.	480.0	400	
D.C. 30 min.	520.0	700	
D.C. 45 min.	900.0	800	
D.C. 60 min.	2000.0	1000	
Shorted by galv. 10 min.	75.0	500	Potential drops rapidly at first and then more slowly
A.C. measurement	430.0	<100	Average of five measurements
A.C. 60 min.	400.0	<100	

was dissolved in 100 c.c. of distilled water. Absorbent cotton was saturated with this solution and was placed between two electrode plates of the type and size described by Ruckmick. They were bound together with waterproof zinc oxide adhesive plaster and had their edges completely covered to diminish evaporation.

The mean data from three or more measurements obtained by passing both direct and 60~ current (0.09 mils.) through such a conductor is reported in Table III.

It was first noted that under these conditions plate electrodes produce a fairly large potential with respect to the liquid ones tested above. For the few seconds involved in making a D.C. measurement they showed zero resistance. But with the continued passage of a constant D.C. they built up a large potential—slowly at first and then more rapidly. The resistance also increased rapidly reaching 1000Ω in the course of an hour. Upon inspection at the end of this time the plates clearly showed, by a change in their surface, where the current had concentrated in passing.

With the use of 60~ current the resistance value was less than 100Ω and too small to be discernible on either the Hathaway or Greenwald apparatus. The potential value decreased 30 μv. in an hour and was only 50 μv. less than that exhibited by the freshly shorted electrodes.

The error in using a constant D.C. with these electrodes over any but very short intervals of time is large. The error in the case of a constant A.C. is undiscovered save that the portion which is due to the NaCl content of the perspiration involved is probably much less than that of the usual measuring circuit. Knowing that NaCl comprises more than half of the solute in perspiration and that ionized

Na and Cl are electrically very active we can only infer that the rest of the solute introduces no greater error.

The DuBois Reymond electrodes. The electrodes with which the data given in Table IV were obtained are fairly typical of the DuBois Reymond electrodes in general use. Two glass cups having mouths 1 in. in diam. were used. Each cup was connected to the apparatus by means of a zinc wire sealed in the glass and attached to a bottom zinc plate, 3/4 in. in diam. The plate in each cup was covered with a paste made of kaolin and a saturated solution of $ZnSO_4$. Contact with the paste covered surfaces of the electrodes was made with cotton that was saturated with a physiological saline (0.7% NaCl) solution.

TABLE IV

Zn, $ZnSo_4$, NaCl Electrodes

Conditions	μv.	Ω
First measurement	180.0	1200
D.C. 15 min.	552.0	1300
D.C. 30 min.	528.0	1400
D.C. 45 min.	516.0	1600
D.C. 60 min.	516.0	1700
Shorted by galv. 60 min.	252.0	1200
A.C. measurement	150.0	700
A.C. 60 min.	240.0	700

As may be seen in Table IV—the values being the averages of three separate determinations—this electrode more than doubled its inherent polarization potential in the course of an hour but reached a fairly constant value upon the continued flow of the current. This change has been shown above to be unimportant in itself as a factor of error in research work on the electrodermal response; but the resistance also increased rather steadily during the hour by about 500Ω. This value represented 50% of the smallest electrodermal response usually reported, *i.e.* 1000Ω. When measured with 60~ current of the same value as the D.C. these electrodes showed only a slight potential increase and maintained a constant resistance for a period of one hour.

If these electrodes are used in an experiment for any considerable length of time in which D.C. is applied, it is highly advisable that the particular model used be investigated regarding its rate of increase in resistance and that corrections be made in the obtained response values. The changes in resistance are unimportant from the standpoint of the current value to which *S* is exposed in the present day 'constant current' apparatus but unless recognized they are likely to be interpreted as changes in *S*'s resistance and thus introduce error.

APPARATUS NOTES

Inexpensive Apparatus for Class Demonstration of the Resistance Response of the Skin

The apparatus herein described is intended for demonstration only and is *definitely not of the accuracy needed for research.* It provides a well tried circuit with fair sensitivity at a modest price.

The circuit used is one originally devised by Darrow.[1] It is a Wheatstone bridge and is therefore measuring essentially *apparent D.C. resistance* changes of the skin. It is one of the best direct current circuits, since it holds constant current strength through the skin and maintains this current at a sufficiently low level to leave the tissue characteristics essentially unaffected by the exosomatic current. It is simple and yet gives a measure of both the resistance level of the skin and of the amount of deflection in terms of ohms. The present hook-up gives no very accurate measure of this resistance but by calibrating the radio potentiometers (C) and (D, Fig. 1) with an ordinary ohm meter and drawing a 10-point scale, a pointer knob can be made to indicate this resistance in rough fashion.

The wiring diagram of the apparatus is shown in Fig. 1, and the corresponding parts are listed. All resistors and potentiometers are wire wound ones obtainable from any radio supply house and can be obtained most cheaply from the mail order ones. The most expensive of these resistors costs a little over one dollar. I.R.C. and Clarostat are suggested as examples of good quality material.

The galvanometer is the most expensive item of the set-up. If the cost of the apparatus must be kept within $40.00, an instrument similar to the Leeds and Northrup No. 2310-D pointer galvanometer with 3.5 sec. period and 1,000 ohm coil is suggested. This galvanometer allows fair demonstration, although the sensitivity leaves something to be desired. The 2310-D costs $25.00. If the greater expense can be allowed, an instrument like the Leeds and Northrup 2420-C mirror galvanometer with 1000 ohm coil and 3 sec. period is recommended. The sensitivity of this is approximately 5 times greater than of the former. The short period galvanometer is essential.

The parts can be easily mounted in a small cabinet with binding posts for the galvanometer connection. The dry cell may be of the radio C 4 1/2 volt type, or if extensive use is contemplated three of the new small 1 1/2 volt ignition type, e.g. Burgess No. 4 FH. All internal connections should, of course, be soldered.

Simple, fairly non-polarizable and convenient electrodes can be made from two sheets of fairly pure zinc, approximately 1 in. x 1 1/2 in. with ordinary rubber covered hook-up wire soldered to the back of the plate. If such zinc is not easily obtainable it can be obtained by cutting out the bottom of an old dry cell. A gauze pad can be made from ordinary bandage and moistened with 1/10 saturated zinc sulphate solution. A more saturated solution than this is not recommended since it has the disadvantage of causing irritation to the skin. The saturated pad should be placed on the skin and the zinc over this. A porous rubber sponge for office stamp

[1] C. W. Darrow, Continuous quantitative records in standard units by the Wheatstone Bridge method, *J. Gen. Psychol.*, 4, 1930, 418-420.

moistening jars will give good and fairly constant pressure if bandaged over the zinc.

Operation is identical with that described by Darrow. Before balance has been obtained the sensitivity of the galvanometer should be reduced by reducing the resistance of the shunt (H). After balancing, greater sensitivity is obtained by increasing H. The circuit alone is first checked by balancing without the *S*. To do this, close switch (S) and adjust resistors C and D until there is no deflection on the galvanometer at greatest sensitivity. The *S* is then introduced by opening switch (S) and adjustments are repeated. The first check-balance setting of C + D minus the rebalance setting represents the apparent resistance of the skin. The resistance response of the skin appears as the deflection of the galvanometer pointer or reflected light spot. The latter is more easily seen by a group of any size and this is therefore another advantage to the mirror type galvanometer.

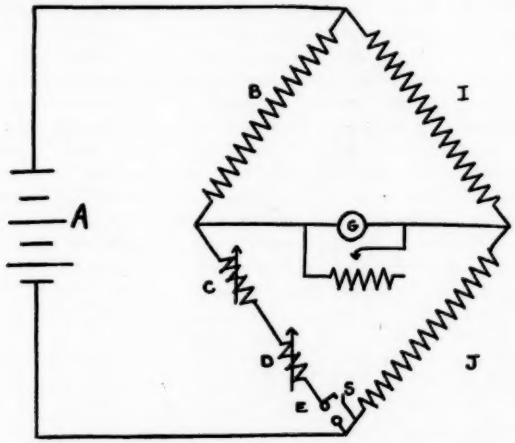

FIG. 1. DEMONSTRATION BRIDGE FOR RESISTANCE RESPONSE
A = 4.5 volt drycell; B = 10,000 ohms fixed; *C = two 50,000 ohm variable (rough adjustment); *D= 5,000 variable (fine adjustment); E = Electrodes, for composition see text; F = 10,000 ohms fixed; G = Galvanometer; H = Shunt (10,000 ohms variable); I = 1,000 ohms fixed; J = 10,000 ohms fixed.

* A 5,000 ohm fixed resistance may be added to this part of the circuit to give a total of 105,000 ohms and insure balancing the bridge without the *S* in the circuit. Such balancing is advisable as a preliminary check of operation of the circuit, and may be otherwise impossible due to the slight inaccuracy of the resistors.

All fixed resistances were I.R.C. wire wound precision type accurate to 1% and all variable resistances were wire wound volume control potentiometers. All potentiometers were supplied with bakelite knobs with pointer.

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NOTES AND DISCUSSIONS

What Is Social Psychology?

A survey of books written on the subject of social psychology reveals a great disorder.[1] In some volumes sociology and psychology become entangled so that neither is viewed in clear perspective and the attempted combination of them results in a patchwork of incoherent excerpts from first one field and then the other. Auxiliary material is brought in from the fields of anthropology, philosophy, history, chemistry, physiology, and other sciences and poured into several hundred pages where the reader is left to orient himself as best he can. This is 'social psychology' at its worst.

The subject is represented at its best in books written by discriminating men who have attempted to establish the position of social psychology in its proper relations to psychology and to sociology, who define their subject-matter and method, and who eliminate irrelevant information. Even so, we still find widely varying approaches to the subject by different authors and, what is especially confusing, inconsistencies still appear within the work of a single writer.

In an effort to discover a systematic procedure whereby order may be brought out of this confused state, several questions have suggested themselves which should be of worth not only to guide the social psychologist in formulating his material but should also aid the reader in discovering the position of a given writer or book on the subject. It is primarily to prospective writers, however, that these questions are submitted, for it seems impossible to produce a coherent and integrated account of social psychology without first making decisions on the following important points.

(1) Shall the approach be made from the side of sociology or of psychology?
   (a) Does the group or the individual organism supply the primary subject-matter of social psychology?
   (b) Is the process of becoming socialized to be chiefly considered or are the results of the process, the social formations, associations, and institutions, of prime importance to the social psychologist?

(2) Is the terminology consistent with some current system of psychology or sociology?

(3) Does social psychology lend itself to experimental methods? If it does not, what methods are best adapted to its problems and aims?

The answer to the first question will probably be determined by the author's own background, while answers to the other questions will be greatly affected by the direction of study indicated in the initial approach. The alternatives stated in (a) and (b) will help to clarify what is meant by the psychological or the sociological approach. The answers to them will be contingent upon the answer to the first question. Thus, if the author is a sociologist and he chooses to base his work upon material with which he is best acquainted he will descant upon the group as it appears in such social formations as the family, the church, the state, and the crowd. He starts with the products of socialization and attempts to discover the facts of the subject by analysing this given result. His is a method of deduction.

On the other hand, the psychologist works inductively. The individual human

[1] Written under the direction of Professor Madison Bentley.

being is the proper object of his investigation, and it is the psychologist's problem to discover how this individual establishes relations between himself and others. The results of these relations, such as the institutions of the church, the family, or a democracy, are not, in themselves, of great moment to the psychologist. They are the inevitable ends of the processes which he investigates.

The two schools proceed from different concepts. The psychologist naturally starts from the individual, a 'general' term as the logician would say, which may be applied to any single member of the class, and the sociologist uses some collective term, such as 'crowd,' 'audience,' or 'family,' which refers only to the group itself. A plurality of individuals is implied, but the collective word has no meaning except as it concerns the whole. Differences in definition would be observed in the sociologist's statement that 'the crowd is a group of individuals *which* . . .' and the psychologist's phrase 'the crowd is a group of individuals *who*. . . .' That is, the description would be attributed to the group itself in the one case, to the members of the group in the other.

So long as the collective concept is employed, only generalizations may be derived. Various kinds of crowds, for example, may be discovered, classified, and the typical conditions which are always present in crowd-formation noted. The observation that a common focus of attention unites the group and that novel stimuli, very large numbers, and strong leaders influence crowd-behavior is typically sociological. As soon as investigation turns toward the *specific effects* of novel stimuli and large numbers, the discourse becomes psychological, for such factors are specifically effective only upon the individual nervous system.

It is as if the sociologist were a foreigner returning from America to his home and describing our food. "There is a certain pastry called pie," he might say, "which is round, so thick, with a crust on the bottom, and berries, pumpkin, or custard in the center. It is served hot or cold and eaten with a fork." His countrymen would be informed and they would be able to recognize a pie; but his description would be lacking in that no one would know how ingredients might be combined, stirred and cooked to produce a pie. The chef who could give them this information might be compared to the psychologist.

Social psychology is logically approached from the psychological point of view. Sociology alone can never get beyond mere generalizations, and to proceed from pure sociology to social psychology would require the introduction of psychological description, otherwise it remains psychologizing sociology. It is our opinion that the psychological description should come in not secondarily but primarily. The psychologist, who already knows the individual, with his apprehendings, emotions, and actions, has the foundations of social psychology within his grasp. He needs plans to determine how the system shall be built, whereas the sociologist starts with the completed structure and hopes to find some foundation upon which it will exactly fit. The psychologist has only to widen a study which has already been started to enter into investigation of those aspects of behavior which are social.

It is not our purpose to belittle the importance of sociology but to recognize it as a necessary guide. If the sociologist can indicate *what* has occurred he has set the task for the psychologist to describe the *how* of the occurrence in terms of the perceptions, memories, comprehensions, emotions and actions of the individuals who compose the group. He must describe what moves an individual to join his fellows,

what he does in the company of others, and how such behavior differs from his non-socialized performances.

It is not to be imagined that various social psychologies can be easily and finally classified by saying that their authors have followed either the sociological or the psychological procedure. Most writers find it inadequate to limit themselves to either one or the other for reasons which have been mentioned: that sociology is needed as a guide and stimulus and psychology is needed for description. However, few have clearly designated the position of social psychology with reference to these two subjects. The books that lay themselves open to criticism are those which are a haphazard combination of the two. Often there are indications that an author with a sound knowledge of one subject has read superficially in the less familiar field and scattered a few terms from it where they seem pertinent. There is no apparent comprehension of their real relationship.

Our second question is designed to remind authors how frequently this careless use of terms occurs. Descriptions in various parts of one book may be found to be couched now in terms of the configurationists, now from some form of functionalism, and again from psychoanalysis. Such lack of consistency destroys the force and point of arguments, and it also betrays the author's failure to make clear, even to himself, the relative importance of the various subjects drawn upon in developing his social psychology.

As for the rôle of the experiment in social psychology, it seems that it cannot be determined until the subject-matter is defined. If the group is taken as the object of investigation, experimentation may become difficult or impossible. It is not a part of the usual procedure of the sociologist, which is one of personal observation, of the collection of testimony, of case-report, and the like. Conclusions are based upon concepts abstracted from these data and there is great divergence in individual interpretation.

The nature of a good experiment must be revealed before it can be decided whether or not material for investigation is adaptable to experimentation. It has been implied that an experiment must take place in a laboratory, but no adequate reason appears for either excluding or necessarily including the laboratory as background for experiment. Socialization certainly can take place in the laboratory, but it is likely to be different from that occurring elsewhere, in that the subjects are tuned to do what the experimenter tells them, his formal instruction becoming an initiating governor.

The problem of establishing a procedure which can be repeatedly performed is a serious consideration for the social psychologist who proposes to experiment. The laboratory offers a place of relative isolation where the greatest number of variables may be excluded and stable conditions obtained, which can be duplicated whenever necessary. As mentioned above, however, the laboratory may not be the ideal place for observing social phenomena. Before the experimenter starts activities on a person or group at large he must weigh carefully the possibilities of controlling many variables.

Before this question can be answered, then, the subject-matter of social psychology must be defined and the essentials of a good experiment determined.

If experimentation is rejected as the main method of the social psychologist we are then confronted with the problem of what this method is to be. The writer must rely upon his own resources as observer, adapter and critic. Many authors

turn to the works of older social psychologists, philosophers, historians, and sociologists, and develop their systems by criticizing these, taking from them what is valuable and substituting new interpretations for what is unsound. This may result in brilliant theorizing or in mediocre summarizing. At any rate, the product is of an abstract character more akin to philosophy than to scientific psychology.

More tangible facts may be found in observation of particular events and in records which may be treated statistically. Such information may be obtained from questionnaires, rating scales, tests, or, in the former case, from testimonies and reports of such social behavior as the anthropologist makes or such as the social-worker records in his case-reports.

The source of knowledge which is used in formulating a social psychology is an important determiner of the validity and value of the work. The relative merits of these different kinds of facts should be weighed.

The more one reads the more one wonders exactly what 'social psychology' may be and it seems that only through decision on the points which we have raised can the topic be defined.

Aside from the fundamental questions which determine the outlines of composition, there is a great variety in the choice of factual materials to be used in supporting the author's thesis. Unless the thesis is clearly stated, it is probable that facts will be chaotically introduced from many sources, some of them utterly irrelevant. This confusion of material distracts the reader of several recent social psychologies. Facts employed to 'fill in' without specific significance notably include phylogenetic or ontogenetic development, animal studies, irrelevant information concerning neurological and physiological functions, and much besides. Why should stories of animal behavior, theories of linguistic development, or diagrams of the nervous system be introduced where their specific relation to social psychology cannot be demonstrated? Certainly, simplicity and conciseness are virtues too often concealed or deliberately obliterated by large amounts of material which is never shown to have a definite use in exposition. Too often are readers first informed of life among the ants and bees, and then led on to other topics with never a reference again to the insects. Too often is a student who has no knowledge of neurology confronted with a plate showing the gyri of the brain when a simple sketch of a reflex arc would suffice for the author's dubious purpose. Some authors evidently consider that it gives their books an appearance of being truly 'scientific' to include complex diagrams and Latin names of nerves and muscles. If the author has a clear conception of the subject which he is trying to expound, there should be no difficulty in integrating his facts and applying his principles, no matter how diverse the sources from which he draws.

It is only natural that such wide and fundamental differences as we have revealed in the approach to the subject should result in extremely diverse treatments. It is a common experience to discover that two books entitled "Social Psychology" seem to be dealing with entirely different subjects. Let us illustrate from recent books. An informing instance may be found in the works of Floyd Allport and Kimball Young.

Allport has recognized that 'social' is the modifying adjective and that his subject is psychology. His terminology, which is that of the psychologist, is for the greater part consistently of a modified behavioristic sort. He writes almost entirely in terms of stimulus and response, but includes 'consciousness' as an accompaniment

to behavior. In employing concepts of other writers, he attempts always to translate their theories into his own language.

The individual is taken as the object of study, for the author conceives it to be impossible for the group to be more than an aggregate of individuals. Study of the socializing process, as we should expect in connection with a psychological study of the individual, is given more attention than the social formations themselves. Instead of noting what a crowd, for example, does and classifying crowds and their characteristics, Allport devotes himself to a study of what occurs specifically in the individual as his behavior is modified by his presence in a crowd. He emphasizes the importance of the circular reflex in perceptions and actions.

Allport uses the experiment as a method of accumulating information, but he does not prove its adequacy. His experiment in the study of "social facilitation" offers many points to be criticized in method and interpretation. As a rule, he confines his auxiliary data to pertinent facts. For example, he uses description of ontogenetic development to make clear his demonstration of the social importance of speech, and he analyzes the act of speaking into eye-ear-vocal 'reflexes,' pointing out the changes which occur as mere vocalization acquires linguistic meaning. His introduction of other historic material, in the recital of experiment and theory, cannot be entirely sanctioned. Too much space is devoted to the discarded work of other authors.

Accounts of animal behavior are included which do not add anything to the description of social functions in the human being. In fact, Allport might be accused of proving inconsistent with his original thesis. He has defined social psychology as a science which, besides studying behavior, "decribes the consciousness of the individual insofar as it is a consciousness of social objects and social reactions." He can hardly carry through a useful description of the consciousness of animals.

This, then, is one direction which social psychology may take, as determined by the primary points which we have raised. Kimball Young, answering our questions differently, has produced a book with the same title of Allport's but with quite different subject-matter.

Young approaches the field from a background of sociology which strongly colors his writing. He writes without a consistent psychological terminology.

To be sure, Young's position in regard to the relative importance of group and individual is not exactly clear. Social psychology he calls "the study of personality as it develops in relation to social environment." We are led to believe, then, that the individual is the object of investigation, yet in the next paragraph the author lays down "as a systematic postulate that, for purposes of social psychological analysis, the group is antecedent to the individual." Without stopping to ask what the word 'antecedent' connotes, we find that the group is evidently taken to be of prime consequence. Only one of the five parts of the book is concerned directly with the nature of the individual.

In the preface the statement is made that a study of the group throws "a more understanding light on the individual's personal behavior" and the reader understands that attention is to be turned to the group simply as it appears in relation to the individual. It is impossible, however, to discover a drawing-together of facts concerning the group and the materials on the nature of the individual. The resulting treatment of the group as the primary subject-matter of the work is characteristically sociological. Young concludes that the product is a more important object of study than is the socializing process. He therefore studies the "content of mind and be-

havior" rather than the processes of psychological function which, as he believes, compose a realm for the psychologist to investigate.

Experimental results are not used in supporting the author's propositions. He advises that we wait "until we can frame our observations under rigidly controlled conditions" before trusting too much to experimental data. He has employed a certain amount of statistics but relies chiefly on case-studies as bases for drawing conclusions.

Since the author is dealing with the 'content' which he discovers in the human consciousness and the modes of behavior which he observes, we find him willing to take what is already existing and leave to the psychologist or others to explain how it arose. He does not seriously devote himself to the development of performances either ontogenetic or phylogenetic. The one exception is his analysis of the beginnings of language in the individual and race.

Young gives little space to the discussion of animal activity which might be called 'social.' He does note an analogy between human and infra-human 'social' behavior and states that "there is some merit in indicating that it is not alone among higher mammals that nature has produced forms of communal living." It is not clear what that merit is as regards social psychology.

Both Allport and Young have deemed it advisable to present certain diagrams and explications concerning the nervous system which are not entirely essential. Allport finds a definite rôle for the autonomic system in his theory of feeling, but it seems that both have included superfluous facts.

Young has written six chapters into a section called "Psychology of Individual Behavior." This includes notes on the nervous system, physiology, individual differences, heredity and environment, habit, emotions, etc. The choice of subjects and the treatment of them seem to be influenced more by tradition than by the logical development of Young's own book. He has stated that social psychology should show how personality "develops in relation to social environment." (The last part of this sentence is superfluous since he defines personality as "the sum total of images, ideas, attitudes and habits of the individual organized in terms of his social participation.") This, of course, means that he is not referring to the typical human being which the psychologist studies, and the step to the typical human being in his physical constitution and psychological potentialities is valid in this text *only insofar as it includes information which will aid in clarifying and explaining the process or product of socialization.* This it does not do.

The discussions which follow concerning neighborhood gangs, the press and public opinion, leadership, etc., continue uninterrupted by any reversion to neurological or physiological bases. They are observations of social formations as the author sees them and their effects on the individual are left for the reader to guess, or at best, are pointed out by the author from common-sense deductions.

It seems that the precedent has been set of inquiring into the phenomena of suggestion, imitation, and the like, into questions of heredity and environment, into neurological structure and function (with accompanying diagrams) and that all subsequent writers of social psychology fear that they will be outdone if they omit these topics. Take, for example, the diagram in Young's book showing the relation of central and autonomic nervous systems: it illustrates no points which are elaborated in the text, and it is practically unintelligible to anyone who is ignorant of the fundamentals of neurology.

In Young's and Allport's social psychologies we find fairly definite points of view. Young is not so explicit as Allport in the statement of his position, but his system resolves itself into that of sociology in search of a psychological foundation upon which to establish the framework of a social psychology. Each author has some comprehension of the decisions which must be made in order to align his facts into a coherent thesis. This greatly aids the reader in making his own valuations. There is some advantage in knowing where the leak is when we go to patch a boat!

Unfortunately a great many students of social psychology have detected flaws in these positions which lean toward extremes and have attempted to obviate the errors by pursuing a middle course. A distressingly large number of books bearing the title 'Social Psychology' may be well organized chapter by chapter; they may contain interesting and thought-provoking observations, but a search for a unifying theme which makes the subjects treated seem pertinent and relevant to each other brings only disappointment. The failure to answer or the complete ignoring of the questions which have been named reduces many books called 'Social Psychology' to a minimum of usefulness as they become little more than handbooks of sociology-and-psychology, and in point of theoretical construction they amount to an absolute zero. No new or valuable material can be developed by a simple restatement of already existing fact and theory, though of course it is quite permissible to take what is needed from any field of knowledge *provided it can be exposed in a new relation.*

Abraham Myerson's *Social Psychology* is representative of the attempts of intelligent, well-informed persons to voice opinions on this interesting subject and of their ensuing collapse into the pitfalls just mentioned. Time and again Myerson commits the error of being eclectic without centering his excerpted material around a unifying theme to give it new meaning. Throughout the book there are many sage observations which are stimulating and interesting but only as they exist in themselves. When the book is read it is impossible to remember the subjects treated, just as it is difficult to remember the miscellaneous items which we view on a table in 'observation' tests; they are only a collection of many discrete units and there is no principle to follow in trying to recall them.

Myerson plunges into chaos deliberately. He definitely rejects both biases of approach which have been discussed and then he refuses, on three counts, to formulate any guiding principle on which to base his work. First, he says, Tarde and his insistence that imitation and suggestion were of prime importance in social psychology, Le Bon and his approach through study of the crowd, Trotter's emphasis on the herd—all simply overwork their points in proposing them as bases for the study of social psychology. Hence, Myerson concludes, it will be better for him to employ a "pluralistic universe of discourse . . . which can be made unitarian only by an over-simplification." This is a criticism which may be seriously considered. Attempts to simplify often result in the omission of essentials and loss of correct perspective.

In the second place, Myerson justifies his 'go-as-you-please system' by the fact that "equally capable workers differ so widely." This, apparently, is simply a recognition of the fact that diverse points of view exist and the assumption that all are correct. An uncritical conclusion, to say the least!

In the third place, Myerson agrees with Sorokin, whose opinion he quotes: "I

find rather useless the dispute about the boundary line between sociology and social psychology. There is no such line." Sorokin is an authority to be respected and if his opinion is accepted then the struggle to define social psychology is over. However, it may be that there is no boundary-line between the two subjects without reducing either the one or the other to non-existence. Can it not be observed that at sunset the sky is red at the horizon and blue overhead? We may truly say that there is no line between the two colors, yet it cannot be denied that a qualitative difference surely does exist. The more one studies any subject the more does one become keenly aware of its interrelation with other fields, but can a subject not still be defined without the sacrificing of this realization?

Myerson's rejection of both the biases we have discussed leaves for him only a middle course by which he escapes making any binding decisions. It is a course which others, unable to classify social psychology under the head either of psychology or sociology, have taken and it has pernicious effects. Myerson's frank admission that he cannot reconcile himself to specific points of view—the alternatives of the questions we have mentioned—may serve to lead the reader into abandoning critical wariness and assuming with the author that his course is the only one left and hence is the right one. There is something disarming in Myerson's unique acknowledgment of certain difficulties as insurmountable and his exuberant strides *in medias res,* but he is severing connections with stable foundations of accepted information and laying himself open to attack from many sides.

Having refused to "rigidly define" his field, Myerson has no right to say that he will take such material as appears valuable "from the great social psychologists . . . from those who are not (even) conscious of this label." On what grounds can he himself be conscious of them under this label if he has no established criterion by which to judge?

He enters the subject from all angles at once; physiology, chemistry, physics, biology, anthropology, history, sociology and psychology. The result is a great agglomeration of chapters from sociology, from psychology, and from neurology, interspersed with paragraphs from one or another of the other subjects mentioned. Unity and order are sacrificed to a desire to attain a broad point of view. The product is something like the work of a futurist who attempts to paint all four walls of one room on a single canvas.

No one system of psychology or sociology dominates—indeed, neither of these subjects is more important than any of those mentioned above. The terminology changes with the topics which are dealt with.

Myerson condemns Allport for writing only of the individual yet he takes "the individual as a starting point" and devotes a good many pages to inquiring into the nature of the individual. When he proceeds to the group he leaves the individual behind and discusses the family, communication, play, etc., in the most general terms. He makes contradictory statements which are most confusing: "So far as we know, only individuals can feel," and on the next page we find "but men in their operations build up a capital of emotions . . . which is stored away, and lives a life separate from the individual. . . . It is extra-human, extra-individual." (The prospect of encountering an extra-individual emotion piques our curiosity.)

Both functions and products of the organism's activities are treated at length, but the relations between them are not apparent. Myerson is interested in visceral

functions and attempts to indicate their social importance. As he takes them up function by function, he points out their connection with certain social products, but when he starts with social products (as in the chapter on "Social Learning and Teaching Process") he fails to mention visceral function.

Myerson does not believe that experimentation is an adequate method of studying social psychology. He says that "social conditions cannot, except in a reminiscent and shadowy way, be duplicated in any laboratory in the world." It troubles him to think that people will attempt social 'experiments' which obviously cannot be repeated nor their results measured.

Myerson's vacillation between individual and group, between function and product of function, and his frequent shifts in terminology make it easy to introduce data which are only vaguely relevant. The reader is not sure where the discourse will lead him next and he is not surprised at encountering many an anecdote from literature, or a brief statement of a philosophical theory. The many sources which Myerson draws from have already been mentioned and excerpts from all of them are fairly evenly distributed throughout the book. In almost every paragraph is injected a quotation or reference from some other volume, often an interesting observation but of no value as pointed illustrations. They are either mere repetition, uncriticized opinion, or remarks which are remote from the subject.

We conclude as follows from our critical study of recent and current 'social psychologies.'

A choice of direction of approach, of subject-matter, and of method seems necessary for the formulation of an integrated presentation of this subject.

An extension of the psychological study of the individual as he establishes social relations presents itself as a desirable entry into the social psychological field. If the study is to be really psychological, it is clear that it should rest upon a firm psychological basis, not upon principles and concepts drawn from the field of social studies.

Allport has written a social psychology following this course and has produced a fairly intelligible and coherent piece of work, limited only by his behavioristic concepts and method. Young has produced a different kind of study from the background of sociology, one which does not clearly differentiate itself from the latter subject. Myerson has followed no plan and has contributed nothing to the development of the study of social psychology. He exemplifies the futility of drawing from a wide reading and reliable sources but without a coherent and systematic treatment.

Confusion exists in the realm of social psychology. A part of the confusion rests upon unclear conceptions of the nature and place of this subject of study and research and in part upon fundamental disagreements and misunderstandings among the general works upon the subject.

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UNITARY TRAITS OF PERSONALITY AND FACTOR THEORY

In their recent monograph on "Trait-Names," Allport and Odbert[1] have rendered a great service to those who labor with the problems of personality. They list in alphabetical order no less than 17,953 names of human characteristics or states.

[1] G. W. Allport and H. S. Odbert, Trait-Names, *Psychol. Monog.*, 47, 1936 (no. 211), 1.

Thanks to Allport and Odbert we now have not only the count but also the classified alphabetical list of concepts which describe individual differences, characteristics, and states or dispositions. Had we not been overawed by the obvious complexity and many-sidedness of human personality before, we should now be brought to the verge of despair by the utter intricacy of it and we should be downcast at the challenge to cope with the problem of analysis and description. It is up to the psychologist as a scientist to put order and significance into this seeming chaos; to reduce to the simple that which is complex; and to describe in economical terms the multifarious aspects of man. It is a little amazing, therefore, to find the authors concluding their theoretical introduction by contending that the list of trait names is not long enough; that what we need is not fewer concepts, but many more. For example, in remarking upon the fact that there are synonyms among the trait names, they say, " 'Synonyms' should not be avoided; if anything they should be multiplied, in order to do more justice to the variety and number of these overlapping dispositions."[2] This predilection for a multiplicity of descriptive terms would not be so disturbing in itself, were it not for the fact that the authors reject with great vigor the opportunities offered by the methods of factor analysis for reducing very materially the number of necessary concepts required for an adequate, economical description of personality, and in doing so grossly misrepresent the aims of factor theory and in general show a lack of understanding of its fundamental assumptions. The present writer hesitates to criticize what is otherwise a very useful and timely contribution. He is in complete agreement with many of the theoretical points. The points of disagreement which are stressed in this short article must not be taken to represent an attitude toward the monograph as a whole.

For the purposes of discussion it is necessary that we be clear in regard to the meaning of a trait of personality and in regard to the accepted factor theories as they pertain to personality. As for the meaning of "trait" in the general use of the term, it is customarily defined as a distinguishing mark or characteristic. This immediately implies *individual differences,* a fundamental fact which Allport and Odbert apparently overlook and as a result are led into error. For how can anything be distinguishing that does not differentiate between one thing and another, in the case of personality, between one individual and another? The term 'characteristic' carries the same implication. The same writers define a trait of personality more specifically as a "generalized determining tendency" of a relatively permanent sort. It is distinguished from a specific mental set or *Aufgabe* of the moment and also from a specific habitual action. With this conception of trait I am in complete agreement. They also make a distinction between traits that are scalable and traits which are not. By scalable traits they mean the more commonly recognized traits such as ascendance-submission, introversion-extraversion (which they do *not* hyphenate) and perseverance.[3] These traits are regarded as scalable because most people have them and individuals characteristically differ in them quantitatively. My own position with regard to this point will be brought out in the subsequent discussion.

Concerning the factor theory as it applies to personality, I shall attempt to give only a general account. There may be differences of opinion as to the way in which

[2] *Ibid.,* 34.

[3] *Ibid.,* 15.

factor theory should be adapted here. I take it that the typical factorist would prefer to speak of 'dimensions' of personality rather than of traits. He would conceive of a dimension as a continuum along which the individuals of a population could be roughly allocated by reason of their having very much of a characteristic, very little of it, or by having some of the opposite characteristic. Most dimensions of personality, at least the non-intellectual aspects of it, unlike abilities, have positive and negative poles. It is perhaps not necessary to assume continua; discrete classes of opposite 'types' could be postulated, but such dichotomous variables seem unlikely on the basis of our past experience. Most factorists would undoubtedly prefer to find the fundamental dimensions independent or uncorrelated. In geometric terms the variables would thus lie at right angles to one another; the axes of the system are then called orthogonal. This simplifies very much the thinking about the factors and the statistical computations connected with them. One factorist at least does not, however, demand orthogonal axes nor do his methods depend upon them.[4] Correlated unitary factors are recognized and axes may be oblique. The number of fundamental dimensions necessary to describe personality adequately in scientific terms is of course unknown. It may require ten or fifteen, or twenty, or even more. The beauty of the factor theory is that what may require hundreds or even thousands of concepts in order to give an adequate literary description can be more accurately done in terms of a relative few when the fundamental variables are known. Each individual is then represented as a point in a multidimensional common factor space. His position is unique for no other individual possesses exactly the same combination of the amounts of the various factors. To say that an individual occupies a point in this hypothetical common factor space implies more exactness than is true in fact. An individual's continually changing personality would best be described as a moving point, or his relatively permanent position as a fuzzy solid, little larger than a point. Since the factors represent rather general and relatively permanent sets, or determining tendencies, to use the language of Allport and Odbert, naturally they do not account for all that an individual does. Fully as important is the situation which activates the individual and his varied repertoire of special habits. Factor theory would attempt only to account for those determiners of action which can be schematized in the form of mathematical variables—the dimensions of personality, including the abilities. With this very limited picture of the factor theory we proceed to a defense of it against the criticisms of Allport and Odbert. I shall list their criticisms under five categories.

(1) The writers in question fail to see that in factor theory an individual can be unique. They say, for example, that "The method (factor analysis) makes the assumption that all people do in fact possess the same basic constitution of personality."[5] Again they remark, "Factors are established through the use of a whole population of subjects, and these factors seldom seem to correspond to the varieties of personal dispositions that are identified by clinical methods."[6] Along the same line of argument is the assertion that "the trait hypothesis does not assume that all men possess the same traits, or that differences between personalities are merely quantitative differences. On the contrary, it contends rather that no two men possess precisely the same trait."[7]

[4] L. L. Thurstone, *The Vectors of Mind*, 1935, Ch. VII.
[5] Allport and Odbert, *op. cit.*, 12.
[6] *Ibid.*, 12.
[7] *Ibid.*, 14.

On general principles, a trait that is possessed by one individual and by him alone is of little scientific interest. Science looks for common elements or similarities and for common variables or differences. As science it is not interested in the unique event; the unique belongs to history, not to science. The writers whom I have just quoted apparently seem unable to see that one individual can differ quantitatively from another in many variables, common variables though they may be, and still have a unique personality. As if to be different or unique an individual must possess a variable all his own upon which no other individual could be placed. It is my contention that one individual cannot be different from another, or even from *all* other individuals at once, without that difference implying a direction and also an amount. At once, then, we have a variable. It would be an extremely rare case, in my estimation, to find all the individuals in the world at the indifference point or zero point for some variable and one individual showing a positive or negative deviation from that point. This is the case that the writers seem to demand when they say that "no two men possess precisely the same trait."

Since, as was just said, any individual difference implies direction and amount, and since traits are individual differences, then naturally we must discover them from populations and not from individuals as individuals. Even the clinical approach, which focuses its attention upon individuals, cannot ignore comparisons of individuals. Any individual's characteristics are his precisely because he is unlike other individuals. Hence populations enter even into the clinical description of traits. If the unitary traits found by the clinical psychologist do not coincide with those found by the factorist, then I should say "so much the worse for the validity of the clinical analysis." It would be a great comfort to the factorist, and it should be for the clinical man, to know that the results of the two kinds of analysis agreed. Let us hope that they do, but I fear that they will not always coincide for the reason that the primary motives of factorist and clinical psychologist are quite different, and so are their methods. The clinician is dominated by the practical motive, to help his patient. His description is likely to be in terms of elements or factors which he can lay hold of practically so as to change the individual for better adjustment to life situations. His analysis results in types of trying situations, worries, conflicts, and ineffective solutions used by the patient. Any differences or traits which he abstracts from this setting will naturally be superficial differences which are to serve as "handles." To the factorist any performance, or difference in performance, is the resultant, not of one determining tendency or causal factor, but of many. The underlying traits implied by an action are several. They are not on the surface. If they were, factor analysis would not be necessary. Unfortunately it is. In a sense factor analysis is an admission of weakness in our powers of observation. We cannot single out our variables by direct observation. The clinical psychologist may cling to his belief that he can do so; that by dint of years of watching behavior and misbehavior parade before him he can intuitively single out the unitary threads of which personalities are woven. Up to the present date we do not find one clinician's intuitions verifying those of another very well; each man's work is much of an art and just so much the less of a science. The factorist will not be alarmed, then, when the clinician's unitary traits do not coincide with his own. He merely offers his own procedure as an empirical and parsimonious technique which should give a genuinely scientific analysis of personality. Whether one factorist's list of primary traits will agree with those of another is still to be seen. If they do not, then we should find a new tech-

nique which will lead to generally acceptable results. Until we do, we have nothing that we can call a science of personality. The clinician should be one of the first to benefit practically from such uncontrovertable results.

Factor theory would maintain that all individuals *do* possess similar structures of personality; that they all possess the same traits, conceived as general determining tendencies, not as specific habits, but to different degrees. Because an individual happens to fall at the zero point on one of the variables does not mean that he does not possess that trait or have the same basic constitution as his fellow human beings. Nor is there anything in these statements that precludes each individual's being unique. His uniqueness consists in his occupying in the common factor space a position which no other individual occupies. There should be no fear that with even a limited number of variables there are not enough positions in this common factor space to go around, as will be shown later in this discussion.

(2) Allport and Odbert reject the factor theory because of a belief that it demands independent factors. They say, "The trait hypothesis, however, differs sharply from factor analysis on the issue of the 'independence' of traits, holding a contrary position, namely that 'pure' traits are not to be expected in mental organization, for in fact nothing in mental life is altogether independent of other influences."[8] As I have indicated before, factor theory need not stake everything on independent factors. It undoubtedly prefers to find uncorrelated variables, but if human nature is not constructed in just that manner, then we should admit that fact. Allport and Odbert seem to make 'pure' traits logically identical with independent traits, in the quotation just mentioned. Factors may be pure or unique in the sense that they constitute continuous variables relatively remote from other variables, but in the case of oblique axes they are not independent. When the same writers say that "pure traits are not to be expected in mental organization" they are probably thinking again in terms of clinical observation. Such direct observation, even supported by intuitions built upon broad experience, let me repeat, is bound to see impure and correlated aspects of personality. Because traits seem to be intercorrelated in surface observations is no indication that pure and even independent variables do not exist underneath. The end of the above quotation, "for in fact nothing in mental life is altogether independent of other influences" is of the question-begging species, or else it is a self-evident statement, depending upon its meaning. If it means that one trait is bound to influence another, then it merely reasserts the main contention of the authors that traits are not independent. If it means that every mental characteristic has causal influences, then no one will probably want to quarrel with it. In the latter case, however, it does not disprove the fact that the causal factors may themselves be independent of one another, which is precisely the typical factorist's position. The factors are the independent, or at least isolable, joint determiners of the superficial differences in personality. These superficial traits naturally seem intercorrelated precisely because they have in common identical factors.

(3) Another false idea which Allport and Odbert seem to hold is that it is the aim of the factorist to synthesize traits. This may be gathered from the following quotation: "It has been found that certain scales purporting to measure 'introversion' have high correlations with other scales purporting to measure 'neuroticism.' Factor analysts, guided by pragmatic convenience, wish to consolidate the overlap in the

[8] *Ibid.*, 12 f.

two scales . . . and to rechristen the new 'independent' factor something suggestive of *both* neuroticism and introversion."[9] Again, "Many scales, it has been found, even though they purport to measure different dispositions, do in fact overlap to a considerable extent, and it is thought that the overlap should be eliminated by bringing all independent variance under a single purified rubric."[10] These two statements, almost identical in intent, highly misrepresent the aim of the factorist. They imply that the overlap or communality existing between two test variables is confined to one common factor alone and that that common factor partakes of the nature of the two purported test variables. That this could happen I am not ready to deny, but a limited experience with factor methods will show that the common ground between two test variables, which are as heterogeneous as the present personality scales, is more likely to result from the existence of two or even more factors held in common by the two tests or scales. Furthermore, the nature of these common factors may require definitions and names that are somewhat unexpected and even remote from the two supposed traits of the two test variables. Factor theory does wish to reduce the many observational variables to the smallest number of conceptual variables, and in the long run this can be accomplished. It may have to replace two test variables with two others, or three or more others; it cannot accomplish its telescoping by fusing two tests at a time. All tests are far too heterogeneous to permit this type of approach. The present writer has recently found that when a questionary supposedly constructed to measure introversion-extraversion is subjected to factor analysis, item by item, no less than five *independent* factors must be assumed and as a rule each item is 'loaded' with two or more of those factors. Very few items are 'pure' in the sense that they are diagnostic of one and only one factor.[11] It follows that when a questionary or scale that is scored for 'neuroticism' and one scored for 'introversion' are subjected to a factor analysis along with many other test variables, they would be found to possess not one common factor, a hybrid 'neuroticism-introversion,' but several common factors.

(4) There is a feeling on the part of Allport and Odbert, made frequently explicit, that a limited number of factors cannot take care of the great multiplicity of individual personalities. They maintain that instead of reducing the number of terms describing personality we need many more; as if 17,953 trait names were not enough. The chemist might as reasonably insist upon using all nouns naming substances plus all descriptive adjectives applying to substances and adding more, rather than to use his periodic table and the properties of the elements therein! This feeling is well illustrated in the quotation, "There are perhaps 300 words in English designating color experience. By tetrachoric correlations this list might be greatly reduced, but what an inadequate vocabulary would result for the profusion of distinguishable color sensations, estimated to exceed 2,000,000 in number!"[12] What a good argument for factor analysis, or some similar technique, this turns out to be. In passing I should question the estimate of the number of distinguishable visual experiences, but that is irrevelant to my argument. The larger the number, however, the more pressing the need of cataloging. The larger number does not

[9] *Ibid.*, 15.
[10] *Ibid.*, 11 f.
[11] J. P. Guilford and R. B. Guilford, Personality factors S, E, and M and their measurement, *J. Psychol.*, 2, 1936, 109-127.
[12] Allport and Odbert, *op. cit.*, 34.

necessarily make any more absurd or difficult the establishment of a simple system of color designation. For years we have designated colors from a scientific point of view in terms of their three attributes, hue, tint, and chroma. The Munsell system and similar systems enable us to identify with great exactness, perhaps not 2,000,000 colors, but any number with which we find it necessary to work in a practical way. If one wishes 2,000,000 color names for literary purposes, then let him multiply names to that limit. I venture to say that when one of the 2,000,000 names is used no one else will know which color he means. Even the common term "lavender" means many things to many individuals. I frequently ask students to identify such colors as lavender in the Munsell Book of Color and they point to colors which are classified under as many as five hues, five different tints, and as many different saturations, all of which might satisfy the name "lavender" for the same individual. The student is thus impressed with the fact that in science we need a more exact system of terminology. He appreciates the reference of a color to the system of hue, tint, and chroma. I should be very happy to see anyone take the suggestion of the writers whom I have quoted above, to apply a factor analysis to a large and varied number of color samples. I think the result could be surely predicted. We should find three independent variables, not exactly hue, tint, and chroma, but a black-white variable, a blue-yellow variable, and a red-green variable. These are the "factors" for color. Every color experience has a point or position in this three dimensional color system. Neutral gray is at the origin. Any color can be described in terms of the projections it makes on the three axes; by giving the amount and sign, whether positive or negative, of the black-white component, the yellow-blue component, and the red-green component. The analogy to personality factors is perfect. Each individual, like a unique color, is represented as a point in the multidimensional system, describable in terms of his projections upon the various axes or variables. If one can describe 2,000,000 colors in terms of only three variables, how many individual personalities can exist in a system of ten, fifteen, or twenty variables? Such a description, I maintain, when we know our variables, is accurate and scientific, economical, and ultimately of practical value. There is no need of multiplying synonyms and shadings of meanings, unless one wishes thereby to emphasize some particular points in the common factor space, points which because of their more frequent occupancy by individuals deserve special attention. Certainly one should not want to invent a name for every point, for he would end up with as many terms as there are individuals.

(5) A final objection to factor theory as voiced by the writers in question is that the variables discovered by analysis are-not 'real' units but artifacts. To quote them on this point, "Since the factors established by this method are wholly dependent upon the scales employed, the introduction of new scales seriously alters the nature of the factorial variables."[18] Naturally the factors must be related to the scales employed. Variations in scores on the scales employed are by hypothesis attributable to individual differences in the underlying factors. No factors can appear in the analysis unless they are held in common by two or more of the scales employed; but the statement which I have just quoted also implies that the same factors are not found except in the same combination of tests. This raises a real issue which we shall have to admit is far from settled. The issue cannot be dissolved by making

[18] *Ibid.*, 12.

dogmatic assertions or by argument. It is a matter for empirical determination and we have not by any means seen factor analysis in action long enough to draw a dogmatic conclusion, either positive or negative. So far as tests of ability are concerned, factor analysis has again and again revealed similar lists of primary abilities, including such factors as verbality, numerical ability, space perception, retentivity, mental speed, and the like. These lists have been derived from the use of varieties of tests applied in various combinations. I should not be willing to assert that the same test will be found to have the same factor loadings with its factors when that test is factored in another battery or when given to a different population. There is nothing in factor theory which requires the same test or scale to have the same factor loadings, or even the same factors, necessarily, in different populations; but in the same population, assuming that the individuals remain unchanged as to the methods with which they attack the same test, the factors and their loadings in that test should remain approximately the same no matter with what battery of tests it is given or factored, provided, also, that there are other tests in the list having those factors in common. The same facts should be true concerning the factors in personality scales, although we have insufficient factual evidence to say that they are true.

As to the more general question, when is a trait 'real' and when is it an artifact, why should psychologists be so greatly concerned? A similar question would be as to whether the atom or the electron is real or an artifact. If individual differences behave in intercorrelation as if they should be accounted for by a limited number of variables which appear consistently from time to time, then we should conclude that we have found a very useful and commonly acceptable mold into which to cast our descriptions of personality. One may be inclined to assert that the wetness of water is more real than the existence of oxygen and hydrogen and their properties, because he can sense directly the wetness whereas it takes laboratory analysis and imagination to grasp the nature of oxygen and hydrogen. There is no question, however, as to the scientific value of the two approaches to the understanding of water. So with the understanding of personality. The so-called factors may be called nothing more than logical constructs, and likewise with electrons and atoms, they can still be tremendously useful. Their usefulness may lie in their utility in pointing to hereditary units, underlying physiological properties of a simple nature, and to genetic factors which make personalities what they are. If they merely help in these larger theoretical problems they will have served their purpose.

University of Nebraska J. P. GUILFORD

ON THE NUMBER OF ARTICLES OF PSYCHOLOGICAL INTEREST PUBLISHED IN THE DIFFERENT LANGUAGES: 1926-1935

Twice before, in 1917 and 1926, the author has published articles on the number of articles of psychological interest published in the different languages.[1] The first article covered the period 1894-1915; the second was a supplementary report covering the 10-yr. period 1916-1925. The present paper is an additional report covering the 10-yr. period of 1926-1935. In each case the materials consist of an analysis by language of the titles listed in the *Psychological Index*. It happens that this report includes the last issue of the *Index* to be published inasmuch as this

[1] This JOURNAL, 28, 1917, 141-150; 37, 1926, 578-580.

journal is to be discontinued with the present 1936 issue. The first report (1917) may be considered to cover the development of psychological publication from the early days of psychology to the World War. The second report (1926) covers the period of the war and post-war years, and this third report (1935) may be expected to show the trends of psychological publication in the post-war period of readjustment and of the depression which grew out of this readjustment.

In the two earlier reports, the number of titles were given for English, German, French, Italian and titles in other than these four languages were grouped under the caption "All Others." In the present paper, it has been deemed necessary to isolate the Russian titles from the more general caption and to report these separately. One source of error, which is eventually self-corrective, is that the titles are listed under the date of publication of the *Index* rather than the actual year of publication of the book or article. There is sometimes, but not frequently, a lag in listing of one year but the error caused by this lag is so small as certainly not seriously to effect trends.

The number of titles in English, German, French, Italian, Russian, and All Others for each of the 10 years are given in Table I. The results for every year

TABLE I

NUMBER OF TITLES IN DIFFERENT LANGUAGES

Date	English	German	French	Italian	Russian	All Others	Total
1926	2374	1650	454	559	448	22	5507
1927	2805	1507	541	451	475	19	5798
1928	2679	1677	394	217	504	16	5487
1929	2725	2283	356	347	592	89	6392
1930	2690	2658	396	331	396	99	6570
1931	2834	2616	584	276	288	194	6792
1932	3089	1063	471	680	329	192	5824
1933	3783	948	456	399	412	288	6286
1934	3540	1099	345	362	423	284	6053
1935	3611	1082	486	104	224	420	5927

are found in successive rows and for each language in successive columns. In the last column will be found the total number of titles in all of the languages. The variation in the total number of titles, which reached a peak in 1931 with 6792 titles, is not totally dependent upon the number of books and articles which appeared during any given year. This total is partly dependent upon the elimination of borderline fields as a result of gradually increasing strictness of editorial policy. The smaller total number of titles during the last 4 years reflects these changes in editorial policy.

The results in Table I have been developed into the series of curves shown in Fig. 1. The first glance at this figure will indicate the marked superior isolation of the curves for English and German. These two curves have relatively high values throughout while all of the other curves have much smaller values.

The trend of the curve of English titles is uniformly upward reaching a peak in 1933 with 3783 titles but falling off very little during the last two years in spite of the more rigid editorial policy of the *Index*. It will be seen therefore that the depression years have had no effect upon the steadily increasing number of psychological titles published in English.

The trend of the curve of German titles is, however, quite different. Going back to the war period, previously reported, an all-time German low was reached in 1917 when only 88 titles were reported. During the next four years, through 1921, the numbers were greater but only once went above 500 titles. From 1922-1925 there was a decided increase in German titles with an average of approximately 1300 per year. During this present 10-yr. period, a dead level is reached

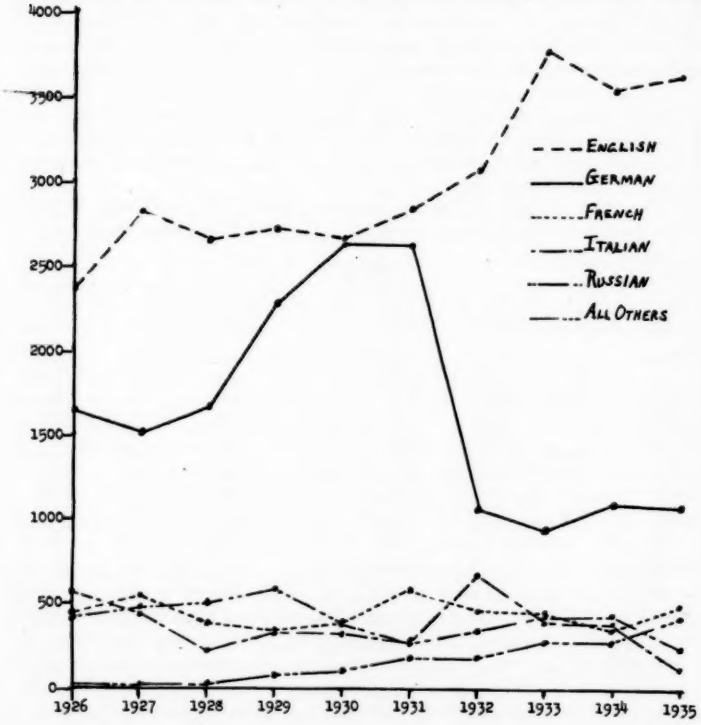

FIG. 1. NUMBER OF TITLES IN THE DIFFERENT LANGUAGES

for the first three years (1926-1928) in the neighborhood of 1600 titles per yr. Then for three years there is a marked increase to over 2600 titles per yr. (1929-1931)—a volume which compares with the just pre-war output. For the last four years (1932-1935) there has been a marked reduction to approximately an average of 1050 titles each year—the number remaining surprisingly constant over this four year period. This recent marked decrease in the number of German titles is probably due, in part, to the general economic situation. Knowing the normal lag in publication, one would not expect an immediate marked decrease in the number of titles

until some time after the beginning of the depression. But certainly part of this marked decrease in the number of German titles is the result of the political changes in that country which have forced so many of the productive German psychologists out of their academic positions.

The curve for French titles remains practically constant during this 10-yr. period with a yearly average of approximately 400 titles. There is no apparent effect of the post-war depression period. The largest number of titles was 584 in 1931 and the smallest was 345 in 1934. But the next year (1935) shows the third highest number of titles for the 10-yr. period. With the exception of one year (1925) there were more French titles each year in this last 10-yr. period than for any year previously.

The curve of Italian titles shows a still different trend. The increase noted during the previous 10-yr. period is continued during the years 1926-1927. But during the last 8 years there is again a falling off of titles (except for the all-time high of 680 in 1932) until only 104 titles are recorded in 1935—a smaller number than in any year since 1917. One suspects that this trend is largely due to economic conditions during the last years.

Until the time of the present report, practically no Russian titles were recorded in the *Index*. In 1926 arrangements were made for an adequate report of the Russian work. During the first 4 years of the present period, there was a steady increase in the number of titles in this language from 448 in 1926 to the high of 592 in 1929. Since that date there has been a decrease in the number of Russian titles with an average of approximately 350 each year.

Before 1926 there had never been as many as 100 titles recorded in languages other than English, German, French and Italian even including the Russian. Exclusive of the Russian, a similar situation is present for the first 5 years of the present period (1926-1930). But since 1928 there has been a steady increase in the number of "All Others" titles in spite of the Russian titles being listed separately. The "All Other" group reaches a high of 420 titles in the year 1935. If the Russian were added to this group, as they were in other reports, the totals for the last 3 years would be: 1933, 700; 1934, 707; and 1935, 644. Part of this increase is to be found in much better editorial service during the last years; but certainly part of the increase is to be attributed to the newer attitude of "nationalism" especially in some of the smaller countries. The "All Others" group this year included articles in Bulgarian, Chinese, Czech, Danish, Dutch, Finnish, Georgian, Greek, Hungarian, Japanese, Norwegian, Polish, Portuguese, Rumanian, Spanish and Swedish. Add English, German, French, Russian, and Italian and one will realize that to cover the field of psychology completely, the poor suffering psychologist may be expected to have an adequate reading knowledge at least of 21 different languages. Formerly practically all psychology was written in one of four languages. Today the trend seems to be in the direction of writing in the language of the country in which the book or article is published.

For purposes of comparison, the totals found in Table I have been reduced to percentages for each year. These values will be found in Table II which has a form similar to the other table.

By an examination of this table it will be found that there has been an increase in the percentage of English titles from the low 40% to the 60% level during these 10 years and a corresponding decrease in the percentage of German titles

from the 30% to about 18%. The percentages for the French, Italian, and Russian titles have remained fairly constant. Those for "All Others" have risen from zero to over 7% during this 10-yr. period.

Twenty years ago the author closed the first paper of this series with the sentence: "Perhaps the most striking point brought out by this study is the extreme necessity for the student of psychology—no matter of what nationality he may be—to have a facile and critical reading knowledge of both German and English." This sentence is as true today as it was 20 years ago, although the present trends might cause one to transpose the order of these two languages. For, in the last 4 years the percentage of English and German titles combined have been over 75% of the titles recorded in all languages during this period. During these 4 years, furthermore, the percentages have steadily increased so that the combined English and German

TABLE II

PERCENTAGES OF TITLES IN DIFFERENT LANGUAGES

Date	English	German	French	Italian	Russian	All Others
1926	43.1	30.0	8.2	10.2	8.1	0.4
1927	48.0	26.0	9.0	8.0	8.0	0.0
1928	48.8	30.6	7.2	4.0	9.2	0.3
1929	42.6	35.7	5.7	5.4	9.3	1.4
1930	41.0	40.5	6.0	5.0	6.0	1.5
1931	41.7	38.5	8.6	4.1	4.3	2.9
1932	53.0	18.3	8.1	11.7	5.6	3.3
1933	60.2	15.1	7.3	6.3	6.5	4.6
1934	58.5	18.2	5.7	6.0	7.0	4.7
1935	60.9	18.3	8.2	1.8	3.8	7.1

titles account for 71.3% in 1932, 75.5% in 1933, 76.7% in 1934, and 78.3% in 1935 of the titles recorded in all languages.

University of Pennsylvania S. W. FERNBERGER

BEETHOVEN'S CONCEPT OF THE "WHOLE"

In the approach to the mental processes involved in musical production we are confronted by phenomena of a complexity, elusiveness, and difficulty of access unrivalled in any other field of psychological research. Composers, as a class, have been most reticent about giving accounts of their experiences; nor need we be surprised at this. For their attention has been quite naturally directed not upon the dissection and analytic marshalling of psychological processes, but upon the construction of scores, of tonal Gestalten. The brilliant exceptions to this, Tschaikowsky being the outstanding one, merely serve to emphasize the general situation.

Although we have only relatively few observations from Beethoven himself, we do have his workshop records: that remarkable series of notebooks, edited by Nottebohm,[1] revealing the composer's inner musical life, frequently enabling us to trace step by step the evolution of his ideas, and demonstrating how he obtained the results that he did from the rawest, most rudimentary substances. These books of sketches furnish a unique laboratory for the psychologist equipped with a technical training in music. They bear testimony to the vast amount of trial-and-error,

[1] G. Nottebohm, *Zwei Skizzenbücher von Beethoven aus den Jahren 1801 bis 1803*, 1924, 1-80.

the gruelling logical manipulations, and the terrific intensity of thought so characteristic of the master.

We will here consider Beethoven's concept of what he called the "whole," pointing out the main features of this famous and controversial question. In a letter of the spring of 1814 to Treitschke, Beethoven said: "I always have the *whole* [*das Ganze*] in my mind."[2] Just what did he mean by this? How may we arrive at some fairly definite interpretation of it?

Well, the first thing that impresses the student of Beethoven is the boundless vitality of his musical creations, their truly evolutionary attributes. In the case of no other musician is it so highly appropriate to speak of the germinal idea. His sonatas, quartets, symphonies, are *organisms*, not structures reminding us of bricks and mortar.

We can do no better than to choose for our working example the Eroica, which is perhaps—certainly as to its first movement—the supreme expression in the entire range of music of developmental continuity and a propulsive force that appears to generate energy out of itself as it progresses. Moreover, a distinct advantage is obtained in selecting this symphony for the reason that a whole sketch book, known as the N80, is devoted to it. Therein, as Graf has been at pains to show,[3] we can observe how the growth of one part of the musical organism is directly followed by increase in another part, how the later emerges out of the earlier. Leichentritt[4] and Lorenz[5] have conducted mathematical inquiries of the greatest moment concerning the structure of the Eroica. Schmitz,[6] to name another, has demonstrated in what manner the principle of contrasting derivation finds application throughout Beethoven's works.

Ernest Newman champions the view that Beethoven did not, like Mozart, begin with a theme, from which the movement almost automatically evolved itself, but that the initial experience was a "dim vision of the totality of what the movement was to express,"[7] and that then Beethoven proceeded to "dig his themes out of this, very much as a sculptor sees the statue in the mass of marble, and gets at it by cutting away as much of the latter as he does not want."[8]

Newman, surely, has offered us a most attractive analogy. Nor can there be any serious question about the powerful influence brought to bear upon the final shape of the thematic material by the dim vision of the whole. Even though the *Gestalt* assume for the time being the form of a mood only, nevertheless it acts as a determiner for the expansion, selection, and modification of the specific contents that at last rise to the surface and become ever more differentiated. Rimsky-Korsakoff,[9] for instance, tells how, when working on Sadko, an indefinable longing for the tonality of F minor came over him, and seemed to lead irresistibly to the composition of Lyubava's

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[2] *The Letters of Ludwig van Beethoven*, Kalischer's ed. (trans. by J. S. Shedlock) 1, 1909, 313.

[3] M. Graf, *Die innere Werkstatt des Musikers*, 1910, 206.

[4] H. Leichentritt, *Musikalische Formenlehre*, 1920, 1-390.

[5] A. Lorenz, Worauf beruht die bekannte Wirkung der Durchführung im 1. Eroicasatze, *Zsch. f. Musikwiss.*, 7, 1925, 409-422.

[6] A. Schmitz, *Beethovens zwei Prinzipe: Ihre Bedeutung für Themen und Satzbau*, 1923, 1-100.

[7] E. Newman, *The Unconscious Beethoven*, 1927, 123.

[8] *Ibid.*

[9] N. A. Rimsky-Korsakoff, *My Musical Life*, 1923, 299.

aria; it was as though the tonality *as such* dictated the melody, harmony, rhythm, and all the rest of the musical structure. Mood, we may say, is the medium wherein the germination of tonal ideas takes place; but what results is not the precipitation of mood. Mood is compulsive in function, and there exists—at least normally—the relation of fitness, compatibility, between affective atmosphere and artistic enunciation. Wagner has expressed himself at some length on this topic.[10]

Louis Schlösser reports Beethoven as saying: "I know exactly what I want, the fundamental idea never deserts me—it arises before me, grows—I see and hear the picture in all its extent and dimensions stand before my mind like a cast and there remains for me nothing but the labor of writing it down, which is quickly accomplished when I have the time, for I sometimes take up other work, but never to the confusion of one with the other."[11] In a memorable description of his experiences, framed in hallucinatory language, and related in one of Bettina von Arnim's letters to Goethe, we read: "Then from the focus of enthusiasm I must discharge melody in all directions; I pursue it, capture it again passionately; I see it flying away and disappearing in the mass of varied agitations; now I seize upon it again with renewed passion; I cannot tear myself from it; I am impelled with hurried modulations to multiply it, and, at length I conquer it—behold, a symphony! Music, verily, is the mediator between intellectual and sensuous life."[12] Mark how he wrote of a picture (*ein Gemälde*) being in his mind when composing: "und (ich) arbeite nach demselben."[13] Very often this *Gemälde* remained for a long interval shadowy, shifting, as the sketches indicate.

No one, of course, will fail to recognize the indispensability of a totalizing affective set and of the 'goal-idea,' however obscure and inchoate they may be, for every kind of creative work. As to the groping about until content and form—the latter term being nothing more than a synonym for coherence, as Edward MacDowell aptly expresses it[14]—fulfill the vague image and *concept* of the whole already formed, we can study this process to advantage in a letter written by Balakirev to Tschaikovsky, in which he discusses his composition of Lear. The general plan, the concept, as a quasi-verbal matter, was first of all present. "I will tell you how I composed my Lear. After I had read the drama, there was kindled in me the desire to write an overture, and I began to plan—for as yet I had no material, my enthusiasm being confined to the project. . . . You must understand that I had as yet no definite ideas at all. These came only later and began to accommodate themselves to the outlined form."[15]

Now, we are convinced that all lines of evidence justify us in concluding that Beethoven's themes are properly to be regarded as so many primordial cell-constellations, bearing within them the potentialities of colossal configurations that finally come to fruition and perfection as *growth* phenomena. This is the thing we wish to emphasize above all others. Examine the Eroica sketches down to their minutest details, study them in their various interlockings and ramifications, follow out the

[10] Newman, *Wagner as Man and Artist*, 1924, 170-171.
[11] A. W. Thayer, *The Life of Ludwig van Beethoven*, 3, 1921, 126.
[12] *Ibid.*, 2, 1921, 188.
[13] *Beethoven's sämtliche Briefe und Aufzeichnungen*, II, 1907, 178.
[14] E. MacDowell, *Critical and Historical Essays*, 1912, 264.
[15] F. Niecks, *Programme Music*, 1907, 416-417. Also see M. Tchaikovsky, *Life & Letters of Peter Ilich Tchaikovsky*, 1924, 108.

evolutionary processes through which the composer's demoniacal constructive power, as Schenker so well calls it,[16] succeeds in bringing forth a living tonal organism—*not a statue.* Then ponder again these words: "It grows and rises up before me." How far, then, can we safely follow Newman's tempting but superficial sculptural analogy? To us it appears as a most misleading, downright dangerous device.

Romain Rolland accords full recognition to the dynamic agency of what he so poetically has named the "revelation as a whole."[17] But he sums up his analysis of the first movement of the Eroica in this wise: "Right from the bursting of the dam by the two imperious opening chords . . . the river begins to flow; and nothing can stop it until it reaches the estuary."[18] Splendidly put. But far more: the symphony is a *germinating,* an *unfolding; not* a precipitate, *not* a condensation such as is taught by the nebular hypothesis, *not* the result of hewing away as with mallet and chisel, *not* something laid bare little by little under a scalpel.

There is, admittedly, the necessity for cutting away in all composition. In Beethoven there is much of this, and also considerable struggling forward in the quicksand of uncertainty. We know how heavily the haze hung over the second half of the opening movement of the Eroica after the first half had gotten under way; this only goes to show how indistinctly the "whole" must have existed at one time in the composer's mind. But he said—and this is most significant for our thesis: "*Revising* is a real strain, compared with which actual composing is easy."[19] Musical sculpturing came unnatural to Beethoven. Heuss[20] has made much of the fact that when an idea —such as Leonore, Cariolanus, Egmont—arose in his mind, he would look through the sketches to discover a live nucleus which held forth promise of *realizing,* through development, the general idea.

Another thing: the interesting question faces us as to why the "wholeness," the *Gestaltung,* of the Beethoven fast movements is of a distinctly higher degree of 'goodness,' as the configurationists would say, than that of the slow sections. Newman[21] believes that Beethoven had the whole before him to a greater extent in the former than in the latter; but this eminent musicologist has not even considered the well known psychological fact that, up to a critical region, depending upon the operation of a complex of conditions, increase of pulsation rate effects a heightening of the 'goodness' of tonal structure. Temporal spacing of and by itself suffices to bring about varying levels of wholeness in the stream of sound. Time here reveals real, dynamic aspects, determining the genetic characteristics manifested by the sprouting and unfolding of the germinal thematic substance which, in a manner of speaking, has been fertilized by the vision of the "whole," and which, bearing within it the tensions and energies pointing toward 'closure,' finally comes to embodiment in such a work as the Third Symphony.

We have said nothing as to the details of Beethoven's methods and procedures of composing; nor would it be feasible to do so in this place. For that is a long story which, under the hands of the ablest investigators, is becoming longer with the years.

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[16] H. Schenker, *Erläuterungsausgabe zur Sonate,* Op. 111, 1900, 64.
[17] R. Rolland, *Beethoven the Creator,* 1929, 66.
[18] *Ibid.,* 78.
[19] *The Letters of Ludwig van Beethoven, op. cit.,* 93.
[20] A. Heuss, Beethoven in der jüngsten Gegenwart. *Zsch. f. Musikwiss.,* 3, 1920, 242.
[21] Newman, *The Unconscious Beethoven,* 1927, 133.

We refer particularly to the monograph by Mies on the sketch books.[22] In this extraordinary research, a model for future undertakings of the kind, there is clearly presented the history of the combat Beethoven unceasingly waged to triumph over the most baffling problems of musical organization. See how he underwent Herculean labors to master the dynamics of up-beat and 'curtain,' of the melodic apex.[23] Observe how he gradually gains the pinnacle of what Gal calls absolute melodic structure,[24] which is defined as purely diatonic, containing neither suspension nor passing notes on the accented part of the bar, and which is found in its glorified consummation in the "Joy" theme of the Ninth Symphony and the fugue subject which opens the great C sharp minor Quartet. Only after delving into these, and innumerable other aspects of Beethoven's creative activities, are we in the position to appreciate the full force of Schumann's remark that genius must be aided by a will of iron.[25]

Clinton, N.Y. PAUL C. SQUIRES

THE THIRTY-FIRST ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The thirty-first annual meeting of the Southern Society for Philosophy and Psychology was held in Atlanta, Ga., on the Friday and Saturday before Easter (April 10 and 11, 1936). The meeting was held under the auspices of Agnes Scott College. As has been the custom for several years, separate psychology and philosophy sessions were held on the morning and the afternoon of the first day, and a joint session was held on the morning of the second day. The annual business meeting followed the Saturday morning program.

The Friday morning psychological session, under the chairmanship of John E. Winter, included papers on comparative and experimental psychology. The papers on animal behavior were as follows: The Selection of Rachitic and Anti-Rachitic Diets in the Rat (C. E. Wilder); Evidence of Color Vision in Rats (Norman L. Munn); The Modification of Goal Gradient Behavior by Means of Variable Preliminary Feeding (C. T. Morgan and Paul E. Fields). The papers in experimental psychology were: The Sensitivity of the Finger Tips to Alternating Electrical Current at Various Frequency Levels (B. von H. Gilmer); A Further Study of Consonance Discrimination (Eugene Bugg); A Description of Interactional Phenomena in Binocular Vision (S. R. Wallace); The Effectiveness of Visual *vs.* Auditory Presentation of Material (Katherine Omwake); Some Preliminary Tests of the Use of the Artificial Larynx with Stutterers (J. M. Fletcher).

The psychological session on Friday afternoon was devoted to papers on individual differences and abnormal phenomena. Frank A. Geldard presided over this session, and the following papers were read: The Validity of Certain Mechanical Ability Tests for Selecting Cotton Mill Workers (T. W. Harrell); Predictive Value of a Teacher's Rating Scale and Intelligence Tests (T. B. Meadows); Variations of the Tetrad Equation (Edward E. Cureton); Judging Intelligence from Photographs (C. R. Garvey); The Control of Body Temperature by Suggestion (M. K. Walsh); An

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[22] P. Mies, *Beethoven's Sketches*, 1929, 1-188.
[23] *Ibid.*, 5-35.
[24] H. Gal, Die Stileigentümlichkeiten des jungen Beethoven, *Wiener Stud. zur Musikwissenschaft*, 4, 1916, 61.
[25] R. Schumann, *Gesammelte Schriften*, 3, 1883, 126.

Experiment on Cutaneous Anaesthesia Produced by Hypnotic Suggestion (F. A. Pattie and Katherine Dionne); The Modification by Hypnosis of Some So-called Voluntary and Involuntary Responses and the Relation of Such Modification to the Theories of Hypnosis (Roy M. Dorcus); The Nature of Paranoia (R. S. Carroll); The Mental and Emotional Status of Sixty Destitute Girls (G. B. Dimmick).

At the philosophical session on Friday morning Marten ten Hoor presided and the following papers were read: The Basis of Hegel's epistemology (Connie Burwell); Epistemological Coördination in Losski's Theory of Knowledge (A. S. Kohanski); Masaryk's Relation to Brentano (W. P. Warren); Undefined Concepts in Postulate Sets (L. O. Katsoff); Religion and 'Natural History' (Fritz Marti); Beyond the Biological Point of View in Aesthetic Theory (Marjorie S. Harris).

The Friday afternoon philosophical meeting was devoted entirely to a symposium on Aristotle, over which Dr. Albert G. A. Balz, the President of the Society, presided. Seven papers were read dealing with various aspects of Aristotle's philosophy and psychology: The Argument from Motion in Aristotle and Aquinas (R. L. Patterson); A Method for the Analysis of Substance (L. F. Kinney); Aristotle's Theory of Human Reason as Related to His Metaphysics and Theory of Knowledge (B. C. Holtzclaw); Some Presuppositions of Aristotle's Psychology (George Boas); The Knowledge and Use of Aristotle Displayed by Nicholas of Cusa in *De Docta Ignorantia* (Anna Forbes Liddell); A Review of Aristotelian Aesthetics (H. C. Sanborn; Aristotelian Causality and Values (L. M. Hammond).

The annual banquet was held on Friday evening at the Henry Grady Hotel, and the presidential address of Dr. Balz was read immediately after the banquet. Dr. Balz' address was entitled "Some Metaphysical Infidelities of Modern Psychology."

The joint session on Saturday morning was devoted for the most part to general papers of interest both to psychologists and to philosophers. The following papers were read at this session, presided over by Dr. Balz: Plato's Philosophy of History (Adam Alles); The Significance of an Examination in Psychology as a Test of Retention or of Grasp of Relations (J. B. Miner); The Problem of Freedom in the Light of the Concept of Maturation (George Kalif); The Operational Meaning of Intelligence Quotients *vs.* their Mystical Interpretation (H. M. Johnson); Awareness and Inference (Marten ten Hoor); The Philosophical Aspects of Einstein's Theory of Relativity (E. E. Richardson).

At the annual business meeting, held at the conclusion of the Saturday morning program, the Society received reports of committees and of the treasurer, acted upon recommendations of the Council, elected officers and new members, and discussed at length the question of a place for the next meeting. The officers elected were: President, Lyle H. Lanier; Secretary-Treasurer, Frank A. Geldard; to the Council, C. P. Heinlein and Fritz Marti. The following new members were elected: Francis F. Bradshaw, John R. Cresswell, C. B. Garnett, T. W. Harrell, George Kalif, A. S. Kohanski, C. D. Leatherman, Frances McGehee, I. R. Obenchain, Robert L. Patterson, H. C. Phillips, S. R. Wallace, C. E. Wilder, and Florene M. Young.

It was voted to empower the Council to select the place of the next meeting, after hearing further about the plan of a group of scientists in the southeast who are interested in establishing a regional branch of the A.A.A.S. The Council was instructed to choose between the University of South Carolina and the place which might be selected for the proposed joint meeting of scientific societies in the South.

Vanderbilt University LYLE H. LANIER

BOOK REVIEWS

An Introduction to Educational Psychology. By COLEMAN R. GRIFFITH. New York, Farrar & Rinehart, 1935. Pp. xiv, 754.

A certain freshness of approach to many ancient problems and the pervasive but never obtruded influence of the author's own significant research work make this a worthwhile book. Students, however, find it as a textbook both dull and excessively difficult. Good students complain that they have to read a paragraph several times to be sure whether a certain position is being rejected or accepted; obviously most students in such a case will not bother.

In his treatment of the Nature-Nurture controversy, for example, Griffith spirals slowly in upon the problem, arriving finally at a statement which seems to the reviewer sound and which, if it stood alone, would be genuinely helpful to the student. He arrives there, however, less by virtue of an open-minded consideration of data than by the cancellation of fondly embraced (or so they seem) but mutually destructive prejudices.

The general point of view is organismic and rather mildly configurational. Unlike the leaders of Gestalt, however, Griffith leans towards an objectivistic rather than a mentalistic position. Indeed his historical treatment of mentalism seems to the reviewer at times downright misleading. He consistently identifies mind, for example, with the substantial soul, without reference to those who have tried with varying success to evade such identification, and in many other instances he presents and criticizes only the weakest forms of the mentalistic position—generally forms no longer professionally defended.

Perhaps the major criticism, however, is to be directed at the author's use of the genetic approach. "If it is at all possible," he says, "we intend to leave no doubt in the mind of the student that there is, perhaps, no branch of psychology that can throw more light upon the problems of education and upon the possible successes of teaching methods than the study of the prenatal development of human beings" (p. 10).

With all desire to be temperate, this seems to the reviewer absurd. Whether large or small classes are better; how to keep Johnny from talking too much; whether Mabel is stupid, lazy, or mal-nourished; how to correct Fred's persistent tendency to misspell all "ea" combinations; what to do about Vincent's shyness; whether extra-curricular activities train for citizenship; how many years a child should stay in school; whether to section according to ability; whether any child should remain more than two years in one grade; whether punishment promotes learning; etc., are we in all seriousness to determine these largely in the light of what we may some day know of fetal development?

Few psychologists, of course, question that our science must be through and through genetic, especially ontogenetic. No one, for example, denies that the events of early life are determinative of later behavior. To say that actual early experiences are important is, however, a very different thing from saying that the principles of *explanation* derived from the study of fetal behavior are the most useful for later stages of development.

Real laws, to be sure, transcend and ignore the limitations of what Lewin calls

the historical-geographic. Thus any law found valid for fetal behavior is valid for adult behavior—but also conversely. This gives no superiority to the explanatory concepts of early life unless from the facts of that stage laws are more easily and readily derived. Is this the case? I see no reason to think so. The infant's behavior is not simple; it only seems so when looked at externally.

As a matter of contemporary fact, however, we have so few real laws that such considerations are not the major issue. Our principles of explanation are but crude first approximations to universal laws and part of their crudity consists in the fact that as they are now stated they are limited in their applicability to certain sectors of behavior or to certain stages of development only. Is there any reason to believe that the approximation to real laws is closer in the case of early than late behavior? Again, I do not think so. It is, as a matter of fact, easier to secure simplicity, freedom from irrelevancies, and experimental control in many fields when adults are studied. Certain fetal behavior is incredibly difficult to control experimentally; and so long as our principles are contaminated by local and temporal circumstances, their application will be more secure in proportion as it is close to the kind of data upon which the generalizations were based. Thus it seems to me that principles discovered from the study of infant behavior (and having a genuine if limited usefulness there) are frequently of small service when applied to adolescent or adult behavior.

Now needless to say, Griffith, in this book (as well as notably in his research activities) often turns directly to the facts of these later periods for his explanatory concepts, but the belief in the explanatory superiority of the genetically prior so baldly stated above again and again comes to the surface to distort his emphases.

Indeed as a general comment it may be said that this book tends to encourage the teacher to look to general principles and theories rather than to the actual conduct of his charges. It does not serve to sensitize the teacher to the infinite variety of pupil behavior. This is partly due to the author's deliberate espousal of a systematic rather than a "practical" treatment. One may record the opinion that this implied dichotomy is largely false. The writer of a good textbook must indeed have a system, but he need not (in that textbook) defend it. Systems do not exist to be argued about—all evidence drawn from contemporary psychology to the contrary notwithstanding—but to provide a framework on which to drape the facts in orderly and intelligible fashion. To this reviewer's taste, the framework in this book seems too obvious, especially for the undergraduate. On that framework, however, Griffith has most expertly arranged a wide variety of data, some of them not often brought into the picture. It has been for me suggestive to read and it remains valuable for reference.

Ohio State University HORACE B. ENGLISH

Growth: A Study of Johnny and Jimmy. By MYRTLE B. MCGRAW. New York, D. Appleton-Century Co., 1935. Pp. xxi, 319.

This study is concerned with the detailed analysis of developing behavior patterns, and it was undertaken in the hope of correlating function with developing neural structure.

Dr. McGraw adopted the co-twin control method of studying these problems; and her book is written about the original pair of subjects, Johnny and Jimmy.

A second pair and 68 other babies have participated in the broader study of normal development; but these children appear only in casual references and footnotes in this book.

Dr. McGraw had these subjects under her direct control at the laboratory 7 hr. a day, 5 days a week from the time they were born until they were 2½ yr. old. There Johnny was stimulated at 2-hr. intervals in a wide variety of activities while Jimmy lay or sat in his crib with never more than two toys to divert him. At regular intervals, however, Jimmy was taken into the laboratory for tests in the acts wherein Johnny was assiduously trained. At the tests, cinema records were made to supplement dictated notes.

Johnny's daily schedule is nowhere included, but for the first 9 mo. or more he had daily practice in the Moro reflex, suspension grasp, inverted suspension, crawling and creeping, erect locomotion, sitting posture, standing erect, rolling, reaching in prone and sitting positions, rotation, and cutaneous irritation by a pin prick. Observations on most of these were carried through to 780 days.

From cinema records of the babies performing these phylogenetic activities outline drawings were made to show postures in various stages of these behavior courses, which so completely baffle description. In general these activities are inchoate and diffuse at their inception, then for a time they are exaggerated and overdone; but before long other developing patterns set checks and bounds, sometimes to the extent of exaggeration in the opposite direction; eventually the antagonistic patterns fuse and crystalize into the perfected behavior pattern. Johnny's daily exercise in these acts gave him no advantage over Jimmy, whose experience was strictly limited, with the exception that Johnny became more docile and willing in the situations. Dr. McGraw's explanatory hypothesis for this fact is that in infancy many rudimentary behavior patterns function first at an infracortical level; later they are replaced by similar activities functioning at the cortical level. During the structural transition there is a lull in the overt manifestations. The longer the gap between the two levels of activity, the greater the possibilities of modifying the final performance through exercise, she believes.

Training in suitable ontogenetic activities began as soon as Johnny's general development indicated his readiness. Sedentary adults may grow weary at the mere thought of swimming, diving, scaling inclines, scrambling off stools, and roller skating all in one day. Such was Johnny's program before he was a year old; and by 18 mo. leaping from stools, shoving graded stools to make stairs for climbing, lugging and piling graded boxes to clamber up for hanging toys, selecting objects by name, by size and by color, and finding six or eight toys previously hidden were added to his daily work-out. Training in all these activities seems to have continued to 22 mo.

It is Johnny's prodigious feats in these lines that won him newspaper headlines. He achieved excellent skill at swimming and diving, scaled inclines of 70°, dropped off a 5-foot stool, skated and coasted down grades, pushed stools as heavy as 60 pounds and arranged them in suitable series for climbing, tugged and lifted boxes of as much as 13 lb. and outdid Dr. McGraw herself at remembering over night where he had hidden eight different objects. Certainly Johnny's achievements demonstrate that babies have tremendous latent possibilities that can be brought out by proper training.

At 22 mo. of age Johnny was given a 2½-mo. recess and Jimmy's training in

these activities was begun. During his months of restriction, however, Jimmy had built up a resistant attitude toward the laboratory and it took two weeks to overcome this. Furthermore he had gained caution which prevented his leaping from stools with the abandon Johnny showed at earlier ages. Discovering the ease with which he could sit down prevented his learning to coast on roller skates, and lack of persistent interest in the lure interfered with his manipulation of stools and boxes. Only in tricycle riding did Jimmy outshine Johnny. Dr. McGraw feels that Johnny's early training at a time when behavior patterns were inchoate gave him an advantage over Jimmy.

With the collaboration of Dr. Harriett Field the boys' training was rounded out by simultaneous experience in a number of multiple stick, multiple string, and basket-pulley problems. In these situations Johnny was not appreciably superior to Jimmy, although his attitude seemed more alert as to how the thing worked. Intelligence tests rated the two boys as almost equal. Health histories were similar; but in weight Johnny outdistanced Jimmy and his appetite and eating habits were better.

Critics may say that Dr. McGraw has added little to methodology in this study. For Gesell and Thompson first used the co-twin method and the Yale group has developed more refined cinema analysis than is attempted in this study; and obviously the graded boxes and problem situations were suggested by Köhler's work on apes. Those with a passion for figures may clamor for tables of success and failure; and for enumeration and "measurements" of Johnny's skill, even in spite of the frequent statement that the manner in which the child performed was a better indication of his skill than the number of successes he made. Others may charge that since these results are at variance with many of the studies on maturation versus training, wherein later training appears more economical of time and effort, further reference to and discussion of these studies seems obligatory.

Nevertheless Dr. McGraw has certainly pushed the infant training régime to an incredibly successful limit. Furthermore her generalizations concerning her two main problems seem logical and adequately demonstrated. I, at any rate, am willing to add to our slowly growing laws of behavior development the principles that behavior pattern originates as an inchoate response, then becomes exaggerated, is inhibited by other developing behavior processes, and finally settles down to an adequate level of efficiency; that phylogenetic activities are little subject to modification; and that modification is greater when exercise is begun at a more plastic stage of the behavior course.

Harvard School of Public Health MARY SHIRLEY

Psychology: A Factual Textbook. By E. G. BORING, H. S. LANGFELD, H. P. WELD, and Collaborators. New York, John Wiley & Sons, 1935. Pp. xviii, 555.

The more one examines representative volumes in the current flood of elementary texts the more apparent it becomes that the perfect text is an unattainable goal. In view of the heterogeneity of content that modern psychology has taken unto itself the completely discerning and conscientious textbook writer would require a lifetime of effort to sort out and organize the materials that might, with the greatest efficacy, be set before the beginning student. The task of discriminating the facts from the mental playthings would itself be an enormous one. The former are so transient and the latter so obsessive that, altogether, the rules of the game of

textbook writing just about bar perfection. There is at present, furthermore, a changing mode in text construction and fashion must be served here as elsewhere. Whereas the text of a score of years ago was primarily a place for revealing a psychological system, the current text aims at the more difficult task of recording and interpreting the facts gained from the laboratory.

The present text has been launched, in view of the foregoing, under entirely happy auspices. It has been recognized that "it is no longer possible for one man correctly to evaluate and critically to select the results of experimentation over more than a limited range" and that the "facts should be presented to the young student of psychology in terms free from the bias of metaphysical presuppositions or of psychological systems." Accordingly the plan has been followed of selecting experts in the several fields of general psychology to supply material for the various chapters, of which there are nineteen. The book is thus not the exclusive product of Boring, Langfeld, and Weld; they apparently served in the author-editor capacity. Sixteen additional collaborators have been called in. The surprising thing, in view of this, is that the book is so uniform in style—this bespeaks efficient editorial effort.

As to the matter of indoctrinating the student, it must be recognized that there is more than one way of setting up a metaphysical bias. There is a method common to early psychology texts—the darkie preacher technique—which "told 'em what it was going to tell 'em; told 'em; then told 'em what they had been told." The first chapter, on "The Nature of Psychology," in which that procedure would have reared its head had it been utilized, devotes no considerable space to the wrangle over 'schools.' To that extent the authors have lived up to the articles of faith set down in the preface. It is to be noted, however, that references to "identity theory," "monism," and "eclecticism" are so placed in relation to divergent views as to minimize retroactively inhibitive influences in connection with the former. However, lasting student prejudices in the direction of one or another "kind" of psychology are not ordinarily inculcated by introductory chapters in textbooks. They are attained somewhat more subtly, albeit simply, by frequent admonishment in connection with the specific content studied. Textbook writers therefore, unless they seek a stupid catholicity, must indoctrinate. The device here, as elsewhere, is editorial inclusion and exclusion. Sufficient material has been included on certain topics, sensation and perception for example, and the exclusions are so obvious, as to effectively place the book in the "experimental" tradition. It admittedly discloses "an uneven balance among the various chapters" but the complaint is made that "this fault, if it is a fault, is that of psychology and not of the authors. Psychological knowledge is much more extended and precise in some fields than in others."

The choice and balance of content, then, will undoubtedly restrict the text's usefulness for those instructors who are accustomed to setting before the sophomore, in Hollywood style, a general pre-view of academic psychology. By those whose prejudices happen to coincide with the authors', considerable enthusiasm is likely to be displayed. The facts seem to be safe and the internal organization is eminently satisfactory. There are, after all, few texts possessing both these excellences.

University of Virginia FRANK A. GELDARD

A History of Science, Technology, and Philosophy in the 16th and 17th Centuries. By A. WOLF. New York, Macmillan Co., 1935. Pp. xxvii, 692; 69 plates.

This is a useful and important book, which deals with the history of science from Copernicus to Newton. It is replete with specific information about men, their discoveries, their apparatus and inventions, the significance of what each did at the time at which he did it, and—not explicitly but by implication—the way in which each fitted into his time and was perforce constrained by the form of its thought and the limitations of its knowledge. The information is just specific enough and full enough to make the book convincing and valuable as a reliable secondary source; more detail would have made the exposition unmanageable and would have lost the perspective; less would have been to catalogue events without letting the reader share the author's insight into their meaning. There are 316 illustrations, mostly of men, instruments and machines. There is a 16-page index. The literary style is excellent; the reader's attention is never diverted from the thought by either infelicity or elegance of exposition.

The scope of the book is indicated by the following paraphrase of its table of contents: Copernicus, Galileo, the scientific academies, the invention of scientific instruments, astronomy (Brahe, Kepler, Newton's synthesis, observatories), mathematics, mechanics, physics (light, heat, sound, magnetism, electricity), meteorology, chemistry, geology, geography (exploration, cartography, treatises), biology (botany, zoology, anatomy, physiology, microscopy), medicine, technology (agriculture, textiles, building, mining, metallurgy, mechanical engineering, the steam engine, mechanical calculators), psychology, social science, philosophy. Thus the title is slightly misleading. Two-thirds of the book (429 pp.) is the history of science. Only one-sixth (114 pp.) deals with technology and only a tenth (65 pp.) is called philosophy or psychology. So *philosophy* could have been left out of the title.

An adequate review of the major portion of this work on the history of science and technology is not appropriate to this JOURNAL nor within the competence of this reviewer. The text makes extremely interesting reading for the experimentally minded psychologist, and it bears in its style the earmarks of care and accuracy.

The little chapter on psychology (18 pp.) will not interest the psychologist. It describes the psychological views of Hobbes, Descartes, Spinoza, Locke and Leibniz for the benefit of students of the natural or biological sciences. Every graduate student of psychology already knows these facts.

On the other hand, the places where the author treats unwittingly of the history of psychology and where every graduate student of psychology might read with profit are the sections on light and sound. There one perceives how progress in the understanding of the nature of light was long held back because of the 'subjective error' of the physicists: they could not see how white light could be complex and composed of colors. How they fought Newton later on that issue! The account in the book exhibits the simplicity and clarity of Newton's experiments with the prisms, when he analyzed white light into the colors and recombined them and established the laws of color mixture. In the section on sound there are similarly interesting items of psychological import, like Galileo's determination that frequency is the stimulus to pitch and his working out of the laws of the vibrating string. These are the places where the germs of modern scientific psychology are found within the body of natural philosophy.

There is no use in complaining that the author of a history of science does not know what scientific psychology is and thus either omits all reference to it or else puts under it the speculative views of certain philosophers on consciousness. Professor Wolf's less competent predecessors have made this mistake, and there is no reason why he, especially with the psychologist's handicap of living in England, should know better. But we can ask him to do better in the future, for he plans additional volumes on the history of the 18th and the 19th centuries, and jolly good volumes they should be if they maintain the standard set by the first. Perhaps he can make them even better by dealing in them with psychology from the modern scientific view of it as a biological science.

He will not need to make much change in his treatment of the 18th century. The problems of sensory psychology and behavior still lay then with the physicists and the physiologists. He need only remember that they were more important to scientific psychology than Berkeley, Hume and Reid. In the 19th century, however, psychology ought to merit separate treatment in the history of science. Let him see that Charles Bell, Johannes Müller and E. H. Weber are placed ahead of Thomas Brown, James Mill and Herbart. Let him give Fechner his due and ignore Bain. Let him treat Helmholtz in his early days for what he really was, the greatest experimental psychologist that we have had. And let him mention Wundt's laboratory, and Ebbinghaus' work on memory, and the whole vigorous movement of scientificizing psychology in Germany and America. The history of science will be taught thus some day. Why should not Professor Wolf do it now?

E. G. B.

Principles of Animal Psychology. By N. R. F. MAIER and T. C. SCHNEIRLA. New York, McGraw-Hill Book Co., 1935. Pp. xiii, 529.

In its account of the non-human animals Psychology is richer than it has ever been. Great activity in the animal laboratory has most notably enriched it, and the products of the laboratory have called for a number of general surveying treatises. In this country the Warden, the Munn, the Tolman, the articles collected by Moss, and now this Maier and Schneirla have been added to the invaluable Washburn. The new book will find its own place. It is written under the prescription of "adaptive response to stimulus"—wisely tempering its general prescription as it meets its facts. It begins with the simpler forms and searches out the extensions and modifications in behavior to be found in the 'ascending' phyla. The plan is to leave out zoölogical, morphological and physiological facts, save where these are significant for the main problem. Neither is an attempt made to exhaust the experimental literature. Representative forms and samples of behavior and of research are chosen. While other men who know the field would probably choose otherwise, the points of emphasis are generally judicious, serving to develop the main theme and still more importantly improving our understanding of animal activities.

The common limitation and weakness of many current treatments of the living and active animal arise from the circumstance that the SR observers and writers, in avoiding mental agencies, ignore the plain fact that most animals live and act in a world which they have individually created, each after its kind, its resources, and its opportunities. That is to say that each is moved by objects, situations, events, crises, anticipated shifts in its momentary condition, unresolvable predicaments, and

much besides that makes the animal's *theater of living*. This is 'the scene' and this is the 'matter-of-fact' *for the animal;* not the 'outside energy' called 'stimulus,' for this is not the thing (as a rule) to which the animal 'responds.' Fearing to make the animal over in his own image, this sort of psychologist has made the animal over into a physicist, who is supposed to translate the world into energies, elemental materials, and movements, and also made it over into a theorizing naturalist who has a passion for 'optimal' and adaptive ends. The creature, then, is run on the physicist's energies (stimulus) and it runs toward an end to which the naturalist regards it as adaptively suited. Two quite unjoined universes of discourse! It is not commonly realized that the admission, into the psychologist's account, of the animal's life *as he lives it* would in no way imply 'mental' powers, processes, or agents; for all the products are obvious products of the body and of its resources and not of a hypothetical mind.

The 'adaptive response' becomes inadequate in the Maier and Schneirla (as it does for most animal psychologists) when the more complex mammalian conduct is approached. Then such agents and powers as 'memory,' 'attention,' 'alertness,' 'experience,' and 'discrimination' are illicitly brought in and 'stimulus' (outside energy) is left far behind both in description and in explanation.

Although the rat has been greatly reduced in compass, perspective might have further improved by a freer use of the recent Munn (*An Introduction to Animal Psychology*), which has helped to restore this popular and engaging creature to a fairly modest place in the psychology of the animals at large.

In view of the great number of factors in, and conditions of, animal performance, from amoeba to ape, a final summarizing, clarifying, and integrating chapter would have enormously increased the significance of the book.

The present writer finds the new text well suited to senior study, in so far as it presents in a clear and fairly simple way typical forms and stages of behavior with a good account of the bodily devices and operations which underlie performance. The tendency to value behavior in terms of adaptation and the 'optimum,' as well as the ambiguous and intemperate use of 'stimulus,' may be disregarded by the reader or instructor who is more realistically inclined than the authors frequently are.

M. B.

Psychological Foundations of Education. By J. STANLEY GRAY. New York, American Book Co., 1935. Pp. 534.

The introductory chapter is a philosophical discussion resulting in the endorsement of physical monism. The rest of the book is almost equally divided between Psychology and Education, theories and principles being emphasized in both cases. The author supports his views chiefly by recent results of experimental studies but sometimes by logic more convincing to a philosopher than to a scientist.

Objective facts of structure and behavior are considered as the sole data for psychology. The organism always acts as a whole and there is no real distinction between heredity and environment since the essential elements, the genes, do not give the same results when differently combined or when affected by varying physical forces. He opposes the "situation response" concept and rejects the laws of learning set forth by Thorndike and others in favor of the claim that all learning is a change

in the whole organism. There are many quotations from leading authors, and at the close of each chapter, references to others. There is an author and a subject index. The book is an able, up-to-date discussion of psychological theories and educational principles, with special emphasis upon objective facts as the basis of reliable conclusions. It is an interesting and stimulating book to psychologists and educators, and a good text for advanced students of education, but not for beginners.

Certain worth while criticisms may be made of purely objective psychology and of some of this author's presentations. Since science deals with events rather than existences, there is no real need to discuss the philosophical problem of whether this world is really monistic or not. Man has always made some sort of distinction between subjective experiences and those that seemed to be objective. Until inductive science developed he was inclined to over-emphasize the importance of subjective states. This led him to regard other persons as like himself, and to endow all living and changing things with similar subjective life: hence he lived in a universe of differing and changeable spirit personalities rather than in a world of objects and organisms behaving according to fixed laws. Science has assumed the latter as true and has made all its progress on that assumption. Experimenters have found that in the field of objective events only is it possible to make sufficiently accurate verifiable and measureable observations to serve as the basis for scientific generalizations and predictions. The objective psychologist is therefore justified in using objective data whenever possible. Is he, however, justified in refusing to consider subjective observations? The present day physician is concerned with the facts of objective structure and functioning rather than with spirits or emotions; yet he saves time by getting subjective reports of pains, and sometimes is guided in part by subjective descriptions. Neither the physician nor the psychologist, if scientific, regard subjective facts as data for reliable conclusions, but the physician uses them whenever convenient. Why should not the psychologist do so? It is often much simpler to use a subjective vocabulary than an objective one. Everyone attaches some meaning to the word "satisfaction," but only a few understand the objective equivalent "chemical activity of a certain kind stimulating introceptors and prioceptors"; and even those few cannot be sure that the whole organism and not merely "the trunk section of the automonic system is differently stimulated" when one has the subjective experience of "annoyance."

In his enthusiasm for certain ideas, the author sometimes overstates, and fails to harmonize inconsistent assertions. He seems to identify thought and intelligence with language, and asserts that intelligence can be taught. Elsewhere, however, he distinguishes intelligence testing and educational testing, and accepts as necessary the testing of language solutions of problems by experiment. He also describes non-language tests of intelligence.

One example of apparently inconsistent statements follows: "The function of education is to train individuals so that they will become productive and actively coöperate with other specialists" (p. 35). "It (education) is a process of training the child to live his own life most adequately" (p. 319). In his enthusiastic advocacy of mechanical causation rather than free will, he makes the surprising statement that organisms do not discriminate or select although machines are known to do so, and later he says the child "selects" from his environment. He admits of no free will of any kind and so ignores the important form of "free will" which by suitable

training may render the individual free to act independently of present external stimuli, but to situations significant for the future.

Leominster, Mass. EDWIN A. KIRKPATRICK

Motivation of Behavior. By PAUL THOMAS YOUNG. New York, John Wiley & Sons, 1936. Pp. xviii, 562.

Young states in the Introduction that "the book is factual in its approach . . . speculation and theory have been strictly subordinated to the presentation of laboratory findings." As a result, *Motivation of Behavior* falls in that group of recent texts which are long on summaries and inventories of the experimental findings and short on the critical analysis, organization, and evaluation of those findings. Emphasis upon experimental data is commendable but the heaping up of facts with little attempt to see them in their relationships to one another is as regrettable as the spinning of theories unsupported by facts.

Young defines motivational psychology as "the study of all conditions which arouse and regulate the behavior of organisms." As a consequence of this attempt to identify motivation and psychology, he finds it necessary to discuss a wide variety of topics, and some of them so briefly as to have little value for the student. In one chapter, Young considers the protean subject, "Direction and Regulation." As might be expected the chapter is a miscellaneous assortment of topics in which the factor of habit is allotted 5 pages, the verbal control of behavior 2 pages, attitudes and their measurement 4 pages, and bodily structure as a limiting condition of behavior 1 page. Intelligence and other abilities, heredity, neural structure and function, and the endocrine and autonomic systems, which surely belong in any chapter on direction and regulation are not mentioned. It would have been wiser to have limited the discussion of motivation in behavior to fewer topics. In the reviewer's opinion, the problem of motivation should be restricted to the study of the psychobiological consequences of privation and deprivation.

"All behavior is motivated" is the first sentence of the first chapter. Any attempt to hoist a single psychological term to the masthead and proclaim "This is the one fundamental concept in psychology to which all others must render obeisance" is certainly premature, and is hardly justified by a later explanation that all the author meant to say was "All behavior is causally determined."

Although the book contains competent reviews of the experimental literature on animal drives, set, reward and punishment, pleasantness and unpleasantness, and social motivation, there are many omissions. Freud's extensive analysis of motivation and Lewin's work are not given adequate space. The acquisition and modification of motives, a topic which deserves an entire chapter, is slighted. The primary drives of hunger and sex as they operate in human behavior, *e.g.* nutritional deficiencies, hunger and revolt, and the effects of castration and sterilization, are ignored. The consequences of loss of sleep and deprivation of oxygen requirements and the motivation for drug addiction and alcoholism are not discussed. A chapter on methods of adjustment to the various primary drives in different cultures would have been extremely valuable. The overlapping concepts 'stimulus' and 'motive' are not examined. 'Purpose,' although it shares a chapter with 'drive,' is never lucidly defined nor its relation to drive made clear. No mention is made of the possibility that conscious and unconscious drives may affect behavior differently. The chapter on emotion is one that might appear in any elementary text. In it one finds, however, no clear

analysis of the relationship between emotion and motivation. Young's reasons for rejecting psychological hedonism are not convincing and the reviewer could perceive no justification for the distinction between factual and causal hedonism.

The book, however, is not without its merits. As a digest of the experimental literature, parts of the book are worthwhile. For the active investigator in the field of motivation, the extensive bibliographies at the end of each chapter are useful. The book may be used as a text in a course in motivation by omitting certain sections. Several chapters, animal drives and reward and punishment, may well be used as collateral reading in elementary psychology.

The author and publishers deserve much credit for the excellence of the printing, which is large and clear, and for the great number and high quality of the graphs and illustrations.

University of Oregon CALVIN S. HALL

Negro Intelligence and Selective Migration. By OTTO KLINEBERG. New York, Columbia University Press, 1935. Pp. 66.

It has been held that the migration of the negro to the north is due to his superior intelligence, that the tendency is for the inferior negro to refuse to migrate, and so they remain in the south. Intelligence measures such as those of Peterson and Lanier in New York seemed to indicate that such selection occurred since the New York negroes showed scores approaching that of the white population. Before 1929 the newspapers of the North and the South had discussed the northern exodus, but most of them, perhaps two thirds, took the ground that the migrants were of inferior type. Klineberg's problem was to find out whether or not it is a fact that selection accounts for the superiority in intelligence measures of the northern negro over those of the southern negro. He investigated first the school records of migrants made before they came north. Secondly, he made a direct attack by means of intelligence tests. He takes into consideration the influence of city and country on the negro child, and of schools, north and south, as environments for the same. These children came from such southern cities as Nashville, Tennessee; Birmingham, Alabama; and Charleston, South Carolina. Examination of the school record of migrant negro children, made in the south before they migrated, furnishes evidence that they are no better than the average negro children in the southern schools from which they came. A rather thorough going testing program indicates that the performance of the negro migrant improves with his residence in the more favorable environment. The number of cases was 786, and the time of school records runs from 1914 to 1930.

The tests administered were the Binet Test, the National Intelligence Test, the Otis Self-Administering Test, the Minnesota Paper Test, and one or two of the Pintner-Patterson Performance Tests. With the Stanford, 362 children were tested; with the N.I.T. and Otis tests, 308; with the various performance tests, 562. The test performances of southern born migrant negroes were compared with those of northern born negroes in New York City schools. It was found that the latter were generally superior to the former in practically all counts for verbal tests—group tests, the individual Binet tests—but not in performance tests, which are supposed to get around superiority due to linguistic ability. Results are not to be taken as very conclusive.

As to the investigator's conclusion that there is no selective migration of negroes going north, if we compare the school records of migrants made before going north with those of children remaining behind, there is always the objection of inadequacy of school records. He faces this criticism fully. The better test, however, is the comparison of the intelligence tests of migrants and those remaining at home when they are available. These he finds consistent with school records.

That the intelligence score of the negro child in New York City does not equal that of the white child is readily accounted for by difference in social status. The investigator is very careful to say we do not know if equating this factor will fully bring up the negro score. In regard to findings as to relations between skin, color and negroid characteristics of subjects showing white intermixture and the test results the investigator is not able to give correlation coefficients. It is desirable that a fuller explanation be given of his Tables 33 and 34 showing roughly such relation.

On the whole the reviewer believes this study to be one of the most conclusive yet made on negro intelligence.

University of Denver THOMAS R. GARTH

Modern Man. By HARVEY FERGUSSON. New York, Alfred A. Knopf, 1936. Pp. viii, 331.

The book is written for a double purpose—"to discover and define the underlying assumptions upon which modern behavior in the Western World is based" (p. 10) and "to formulate a hypothesis as to the determinants of human behavior which is consistent with contemporary knowledge on the one hand and applicable to contemporary experience on the other" (p. 18). The author has thus "attempted to set forth a complete theory of individual and group behavior" (Preface). The book is divided into three major divisions: The Illusion of Choice, 165 pages; The Ethic of Balance, 127 pages; and The Growth of Consciousness, 32 pages. A brief index and a bibliography of 41 titles are appended.

In the first division the author elaborates the thesis that one fundamental assumption upon which modern man explains his behavior is what may be called the illusion of choice—the notion that his destiny is partly determined by conscious volition. Fergusson holds that "this assumption is not useful or workable in the modern world" (p. 14).

The Ethic of Balance is the central theme of the second division of the book. According to this principle, as elaborated by the author, the individual tends to gravitate toward and join that particular size of social group for which he is by nature and training fitted.

The third division of the book, The Growth of Consciousness, presents the thesis that "Growth of individual consciousness . . . occurs only when reflection (upon conduct) takes the form of criticism and revision of belief in the light of experience, rather than of an effort merely to comprehend the experience in terms of a belief which remains unchanged" (p. 308).

The book is weakened for the serious student by the author's seeming lack of familiarity with the extensive literature on the central problems of his book. He accepts without question, for example, the principle of determinism the validity of which is, and always has been, seriously debated by philosophers and scientists.

The Ethic of Balance is a problem as old as philosophy itself as is also the problem of the nature and rôle of consciousness in behavior. This lack of familiarity is the chief criticism which may be aimed at the book. To the reviewer two minor criticisms are the author's unorthodox psychological terminology and the somewhat repetitious nature of much of the discussion. Even the layman, the modern man for whom the book is written, will find the book none too satisfactory because of the author's frequently abstruse style.

The chief importance of the book rests upon the fact that the author is a keen observer, and that he puts old problems in a new and suggestive setting. The reviewer feels, also, that there is a definite place for a book that attempts to picture in simple, direct language the basic assumptions and principles underlying the behavior of modern man.

University of Arkansas. R. H. WATERS

General Psychology. By FLOYD C. DOCKERAY. Revised edition. New York, Prentice-Hall, 1935. Pp. xxix, 576.

Dockeray has very largely rewritten this new edition. The most obvious change is the division of the 16 long chapters of the first edition into 32 shorter chapters, organized into 11 general sections. The reviewer finds this change an improvement from a practical point of view, for it will undoubtedly simplify the use of the book in elementary classes. At the beginning of each general section the appropriate references to the *Student's Guide* are given, for the benefit of those who use the workbook.

To deal with other rather incidental details first, we might comment on the excellent index, the large, clear print, the improved clarity of paragraph headings, and the generally good format. Unfortunately, the reproductions of the photographic illustrations are decidedly poorer than in the first edition. Graphs and line drawings remain clear and well done, though the total number of illustrations is reduced from 105 to 94. The introductory chapter—a paternalistic "How to Study"—is entirely new. Bibliographies, given at the end of each chapter, in general are short. There is a good, though short, chapter on statistical interpretations which is not commonly found in an elementary text. The emphasis throughout is upon objective, experimental material.

The first section, on the scientific point of view, has three chapters—the problem of psychology, the scientific method in psychology, and the experiment. It is particularly emphasized that to name a phenomenon is no explanation of it, and that in the end explanation consists merely in describing the immediately preceding events in a series. The author's point of view, as in the first edition, is essentially behavioristic; for him psychology is the study of human behavior. A very well expressed distinction is drawn between observation and inference, and the significance of hypotheses as opposed to facts is analyzed. A reaction-time experiment is described in some detail as an example of experimental procedure, but one misses the concise little table from the first edition, of results from 11 different reaction-time investigations by different experimenters, using different stimuli and different parts of the body.

The reviewer has two chief criticisms to offer of the book. One is general, and true of the first edition as well as of this one. Seldom is a precise definition given of the technical terms used. Oftentimes because of the clear style and the good

illustrative material it is possible, even for the beginning student, to determine with some precision what the author means by a given term; but this cannot always be done. In the introductory chapter Dockeray emphasizes the necessity of using the technical vocabulary of psychology correctly as a prerequisite to a real understanding, since many psychological terms are casually used in daily speech, but he himself offends by leaving the student to assume that these terms have the same significance in psychology as they do in ordinary conversation. The second criticism is of the rearrangement of the chapters on sensory discrimination which appears to the reviewer as not being an improvement. In the first place, the descriptions of the physiology of the sense receptors have been taken for the most part out of the chapters on discrimination and placed together in a chapter on "Stimulus and response," in the section on "Types of simple behavior," which also includes a chapter on reflexes. In all cases, the descriptions of the receptors have lost so much detail that they are no longer entirely adequate. There is a lack of unity because the physical stimulus for each receptor is described partly in the chapter with the description of the receptor and partly in the chapter on discrimination. The description of visual sensory experiences as well as of the visual stimulus is no longer the complete and adequate description that it was in the first edition; *e.g.* hue, tint, and chroma are not mentioned, nor is their relation to the physical stimulus discussed. The description of the auditory stimulus remains, though it too has lost something of completeness.

The emphasis throughout the chapters on discrimination is upon space perception. The discussion of optics, of binocular perception, and of auditory and cutaneous space perception are unusually complete for an elementary text. Olfaction, gustation, and cutaneous experiences (except for space perception) have been almost completely eliminated.

The organization of the chapters on learning is much improved, all principles, *e.g.* being discussed in one chapter irrespective of whether they have been discovered from animal or human experimentation. The laws of learning, and particularly the law of effect are analyzed from a configurational point of view; the dynamic aspect of learning is emphasized throughout. This point of view appears again in the very good chapters on observation and attention.

Skidmore College DOROTHEA E. JOHANNSEN

Principles of Abnormal Psychology. By EDMUND S. CONKLIN. Revised edition. New York, Henry Holt & Co., 1935. Pp. xiv, 427.

This text assumes that the reader is already familiar with the biological foundations of psychology and that the more strictly medical considerations are to be studied in the field of psychiatry. This leaves the author free to concentrate upon the more strictly psychological task of describing and illustrating the major facts and theories of abnormal mental processes and behavior. The list of such topics is unusually complete.

Following a survey of general concepts is a new and valuable chapter on "Psychogenic Interpretative Concepts," which presents the organic and functional approaches, maladjustment, complexes, and the numerous mental mechanisms such as rationalization, and projection. Chapters on sensory and mental abnormalities illustrate the gradation of normal into abnormal processes. The student is then introduced to the minor disorders by the two chapters on "Psychoneuroses," and one on

"Multiple Personality." Another chapter on "Speech Abnormalities," and one on "Distorted Thinking" are followed by three chapters on the "Major Forms of Abnormality" (omitting the rarer organic psychoses). These chapters represent a new order of topics and a considerable expansion from the first edition.

A partly new miscellaneous chapter on the "Abnormally Endowed" discusses feeblemindedness, genius, and constitutional psychopathic inferiority. These important topics might well be separated and expanded in a later edition. The well known chapter on "Sleep and Its Abnormalities" is brought up to date, and is followed by a new chapter on the "After Effects of Epidemic Encephalitis" (sleeping sickness). Chapters on "Hypnotism and Suggestibility," "Dreams," "Mental Effects of Drugs," and "Spiritistic Phenomena" are brought up to date and reflect the author's systematic bibliographical work in this field. A new chapter on "Psychotherapy" presents these methods largely as an aid to the understanding of abnormal behavior. The "Historical and Biographical Background" remains as the last chapter.

Students using the first edition have manifested considerable serious interest in sex adjustments, on which topic the present edition and most other texts are still very brief. Aside from this and perhaps the omission of a specific chapter on minor emotional maladjustments, the reviewer considers the revision to be a very clear and representative survey of the field, well adapted to the level of advanced undergraduates and graduate students. Users of the first edition will welcome two improvements in the printing, the excellent typography and the introduction of bold face headings for major topics. Up to date references after each chapter provide opportunity for further individual work.

University of Southern California ROBERT H. SEASHORE

General Experimental Psychology. By ARTHUR GILBERT BILLS. New York, Longmans, Green & Co., 1934. Pp. x, 620.

According to the author's preface, the title of this book is to be taken literally—that is, the discussion is confined to normal, human adult psychology, and the citation of experiments is its principal style. In spite of the danger involved in applying labels, especially ones that have been fought over in the literature, it seems appropriate to call the present exposition 'functional' in a broad sense.

The author also makes a case in the introduction for the explanatory treatment of his material and follows it out, in some measure, in the body of the text. That his interpretation of the term "general" is somewhat specialized is obvious from the space given to various topics. The book is divided into six parts which follow closely the traditional treatment of the subject. After a concise Introduction, Part I treats of "Sensation" (103 pages); Part II, of "The Perceptual Process" (52 pages); Part III, of "Learning and Memory" (169 pages); Part IV of "Association and Thought" (20 pages); Part V of "Work and Fatigue" (144 pages); and Part VI of "Emotion and Affective Processes" (50 pages). The author adds an appendix on statistics and one on psychophysical methods. In Parts III and V, "Learning and Memory," and "Work and Fatigue," the treatment is so complete as to approach the encyclopedic, with the citation of something like 330 titles on "Learning" and 252 titles on "Work and Fatigue." Since it is distinctly a textbook it might well be used as the basis of an advanced course in either of those two fields. The selection which the author found necessary in other topics in order to make possible what he considered an adequate treatment of the chosen fields, has resulted in sketchy discussions

in many places and detracts, therefore, from the "general" aspect of the book. For example, the color pyramid is dismissed with the comment "The main defect with the figure is in the arrangement of the colors in the base relative to one another" (p. 44). Of the qualitative attributes of tone, only pitch and volume are mentioned. Henning's smell prism is pictured, but without comment. Perception is limited, as we noted above, to space perception with three pages on the perception of time. There are no references dealing with the latter. Attention is reduced to two pages in the chapter on "Response Processes."

The treatment throughout seems to the reviewer to be markedly lacking in systematic unity. Experiment after experiment is described in more or less detail without its systematic psychological significance being pointed out or evaluated in any way, unless the inevitable physiological hypothesis be considered the systematic aim. This has its advantages, of course, in that it permits a user of the book to make his own interpretations, but one would like to know just what the author's opinion of the significance of certain results may be and why, in some cases, they are psychological at all.

Hobart College FORREST LEE DIMMICK

Art and the Life of Action: With Other Essays. By MAX EASTMAN. New York, Alfred A. Knopf, 1934. Pp. iv, 227.

To the psychologist, the long essay which gives title to the book is provocatively interesting, presenting as it does the most psychological of current views of art appreciation. For the disjuntion of the esthetic from the merely behavioristic or physiological, and also from the logical and metaphysical, is cleverly and completely effected. A disciple of Dewey, Eastman identifies consciousness with the blockage of impulses to movement. Therefore, of necessity, the heightened consciousness which is the core of the esthetic is incompatible with the immediate direction of conduct toward a goal proclaimed by Nazi and Soviet propagandists as the true essence of all art.

In spite of his communistic (or humanistic) leanings, Eastman is the keenest and wittiest of critics of current Russian warping of the tenets of Marx and Lenin. Preposterous as is the project of turning back the clock by identifying art with propaganda, the premises on which this Neo-Marxian esthetic rests—the appropriation by the revolutionary movement of the whole Hegelian universe and course of destiny—are equally without warrant. Great art, Eastman points out, by communicating experience in all its variety, tends to promote a universal human solidarity rather than partisanship in any isolated historic movement, or proletarian versus bourgeois sympathies.

Arguing for the impractical and non-purposive in art, the author collates (p. 7) an exhaustive inventory of the ulterior sanctions which reactionaries down the centuries have devised to align art again under the regime of morals. Along the way, he takes a fling not only at hedonistic esthetics, at the modern "Cult of Unintelligibility," at the pedestrian sanctions solemnly proposed by the schoolmaster Aristotle and his modern exponent I. A. Richards. He also assesses as "negative and pathetic" the frantic effort by the "disease-loving doctors," the psychoanalysts, to set up a new utilitarian sanction in wish fulfilment—thus annexing art to the Department of Public Health.

Of the shorter essays at the end of the volume two in special are of interest; carrying furious if hilarious attacks on Ernest Hemingway and Floyd Dell—the former

for his attempt to identify the Spanish bullfight with classic tragedy, the latter for his complacent elimination of art (along with insanity and crime) from the pattern state (in *Love in the Machine Age*).

Cornell University — ELSIE MURRAY

The Diagnosis and Treatment of Behavior-Problem Children. By HARRY J. BAKER and VIRGINIA TRAPHAGEN. New York, Macmillan Co., 1935. Pp. xiv, 393.

The book is an outgrowth of the experience of the senior author as director and the junior author as mental examiner in the psychological clinic of the Detroit public schools. The chief purpose of the authors is to present the Detroit Scale for the Diagnosis of Behavior Problems, which consists of 66 items chosen because of demonstrated relationship to manifestations of behavior problems. The items represent health and physical factors, personal habits and recreational factors, personal and social factors, parental and physical factors of the home, and home atmosphere and school factors. The whole is not truly a scale, but rather a list of items which experience has shown may well be made the systematic basis for gathering data upon which a diagnosis of problem behavior may be made. Each item contains questions to children and to parents, additional questions and observations designed to bring out the facts relative to each, and a system of scoring involving five grades of each item with score values from 1 to 5.

The authors present the scale "with the hope that it will aid in removing diagnosis and treatment from the realm of subjective opinion." It seems to the reviewer that what the authors have succeeded in doing rather, is to set up a pattern for the gathering of pertinent data on the basis of which the practitioner can more reliably exercise whatever ingenuity in diagnosis and treatment he may possess.

The chief contribution of the book appears to be the carefully selected list of factors to be investigated together with their interpretation, which constitutes considerably more than half the book.

The book should be very helpful to workers who wish to get as adequate basis as possible for diagnosis and treatment of known cases of problem behavior; to those who wish to discover any who are likely to become such; and also to any one wishing a guide for gathering data to serve as basis for good understanding of any child for whatever purpose. There is a workable index and helpful lists of references follow the chapters.

Cornell University — PAUL J. KRUSE

Self-Instructional Manual in Handling Test Scores. By RICHARD S. HARTER and C. H. SMELTZER. New York, Henry Holt & Co., 1933. Pp. 57.

Each of the major categories of elementary statistics is briefly explained, graphically illustrated, and further developed and clarified by means of one or more well-chosen problems. All told, there are 26 problems for each of which appropriate materials are presented in convenient form. Each problem is followed by a list of questions which direct attention to significant aspects of the data and call for answers to be listed in a parallel, detachable marginal column. These answer strips furnish the instructor a means of checking the work of students.

As further aids to the instructor some final tests are provided, that are inclosed in a sealed envelope which is firmly bound into the manuals. Speaking of this feature the authors say, "Three forms of a test, each approximately equal in difficulty, accom-

panies each book. It is optional with teachers as to how they prefer using these tests. The authors suggest they be collected by the teacher and used at a time suitable to instructor and students." Several forms are supplied in case a teacher desires to require the students who did not attain a certain standard, to repeat the test at a later date. The authors have set a standard of four or less errors out of the thirty possible points on each test.

The manual is very well written and there is no doubt that it will render a genuine service to beginning students in educational psychology without adding appreciably to the work of the instructor.

Kansas State College J. C. PETERSON

Occupational Ability Patterns. By ARTHUR F. DODGE. New York, Teachers College Contributions to Education, No. 658. Columbia University, 1935. Pp. iii, 97.

This book is an historical and experimental study, which traces the origin of methods for portraying ability patterns from the pioneer work of Wundt and Galton, to America through Cattell, to various types of ability patterns used by Gemelli, Lipmann and Ulrich, Viteles, Martin and Trabue.

The experimental portion consists of a study of the records of 651 cases, representing 13 occupational groups, selected from the records of the Adjustment Service of New York City. The occupations were draftsmen, engineers, teachers (high school and elementary), secretaries, stenographers, accountants, bookkeepers, office clerks, sales people (retail and traveling). The test scores were: Pressey Senior Classification, O'Connor Vocabulary, Minnesota Vocational Tests for Clerical Workers, the Bernreuter scores. The object was to determine the ability patterns peculiar to each occupation. Two methods of securing patterns were used: (1) by comparing median test scores of various occupational groups with median scores of the entire group, (2) by comparing scores of each group with norms from the total adult population.

Both methods demonstrate reliable differences and consistencies between dissimilar and similar occupational groups (respectively). Differentiation of individuals of one group from individuals of another group was impossible, however, because of the great amount of overlapping of scores. This result does not encourage the use of these devices for vocational guidance. There is no significant correlation between scores and salaries of the Engineering group, but salaries and years of experience show and r of $+0.42, \pm .08$.

The author suggests that a single standard of norms is better than patterns based on separate norms for the sexes. Occupational ability patterns should be based on minimum scores necessary for success, rather than on median scores.

The study is painstaking, well organized, and searching.

Wells College C. O. WEBER

A Modern Book of Esthetics: An Anthology. Edited with introduction and notes by MELVIN M. RADER. New York, Henry Holt & Co., 1935. Pp. xxxv, 504.

This book might equally well be entitled "Readings in Esthetics." It has, however, a distinct advantage over the usual collection of reprints from original sources. Instead of giving short samples from many authors, which are more likely to confuse than to instruct, the editor has chosen long excerpts from only thirty-one authorities.

The book is divided into twelve parts. Each part deals with an important class of theories in esthetics and is represented by from two to four advocates of the class of

theories in question. We find among the topics treated: the play theory represented by Lange, and Gross; voluntaristic theories represented by Nietzche, Freud and Parker; the emotional theories of Véron, Tolstoy, and Hirn; hedonistic theories by Marshall and Santayana; theories of empathy by Lipps and Lee; theories of psychological detachment by Bullough and Ortega; and theories of isolation and equilibrium by Münsterberg, Puffer and Ogden, Richards and Wood. These are the selections which will probably be of particular interest to psychologists. It is not difficult for a critic to suggest certain authors he would prefer to include, but I believe that on the whole Dr. Rader has made an excellent selection.

Preceding each section is a short introduction by the editor which gives the reader a clear and sharply etched picture of the points of view represented in the respective sections. There is also an introduction to the book by the editor which acquaints the reader with some of the principal concepts and problems of esthetics. Some of the descriptions are tautological, as for example, "Values are expressed in the creative activity of the artist whenever he provides the objective occasion for esthetic valuation," but apart from this fault the introduction gives the reader a very good start in the study of beauty. At the end of the book there is an extensive bibliography for each chapter.

Dr. Rader has given ample evidence in this book of a scholarly and comprehensive knowledge of the literature in esthetics. It is a valuable text for those courses in esthetics which combine the philosophical with the psychological approach to the subject.

Princeton University HERBERT S. LANGFELD

The Improvement of Reading. By ARTHUR I. GATES. Revised edition. New York, Macmillan Co., 1935. Pp. xvi, 668.

The revised edition of *The Improvement of Reading* is a combination of a manual of directions for diagnosing reading difficulties and a presentation of descriptive remedial procedures. It is designed for the first eight grades and deals with all types of difficulties, from the mildest defects in techniques or word recognition to the most serious disabilities. The study begins with a description of the "General characteristics of the diagnostic program" and proceeds progressively from a general class diagnosis to a study of specific cases of reading deficiency, and finally to a consideration of extreme cases.

The author has not made the mistake (quite often made by students writing on a specialized educational problem that is wellnigh exhaustive in scope) of bringing in a great deal of material relating to (but to a large extent extraneous to) the major topic of the discussion. There is a large amount of illustrative materials that are sufficiently practical to serve the teacher in connection with the reading activities of the school. Included among the procedures for diagnosing reading disabilities are tests especially designed to study the physiological conditions affecting reading. There is included recent data and modern diagnostic techniques relative to the physiological condition along with constructive suggestions for the remedying of existing conditions. The problems of *readiness* and *prevention* as they relate to educational growth are assuming an ever more important rôle and should probably have received a greater emphasis. These may, however, be considered as not belonging to the major problem of this volume.

In the Appendix there is rather complete bibliography of reference materials pre-

sented and classified. This consists of 162 pages and presents in addition to the bibliography: (1) directions for administering and scoring the tests; (2) norms for the diagnostic tests; (3) a list of books useful in remedial instruction in grades three to six, inclusive, and (4) questions and exercises on the fifteen chapters of the book. The completeness of the discussion of the procedure involved in administering and scoring the tests along with the presentation of norms for the tests provides a means of arriving at a rather complete inventory of the pupil's capacities and techniques involved in reading. It is especially in this respect that this revised edition is superior to the earlier one.

Teachers and other students of the problem of education will find this volume a most valuable contribution to the literature relating to the improvement of reading.

North Carolina State College K. C. GARRISON

The Psychology of Human Behavior. By JOSEPH H. GRIFFITHS. New York, Farrar & Rinehart, 1935. Pp. xxi, 515.

This is an introductory text planned for use in a one-semester or longer course. The book is divided into six parts. The first part is a brief historical statement of the development of psychology as a science and of the methods of research and study used in this field. The second part deals with intelligence and learning; the third, with feeling and emotion; the fourth, with thinking and imagining; the fifth, with perceiving; and the sixth, with personality.

Much of the historical introduction is a mere enumeration of contributors with limited explanation of their conceptions or contributions. If the text is used by a well trained person this method of treatment should offer no difficulties, however. The experimental studies of animal learning are evidently selected for the purpose of illustrating the author's concept that animals learn by insightful adaptation. He interprets the trial-and-error responses of an animal or human being in the early stages of learning as a process of getting oriented to the problem and thus the initial stage of a process which takes place by insight. In this view he takes issue with Thorndike who considers the hypothesis of trial-and-error learning and that of insightful adaptation in learning as incompatible (p. 82). The chapter on "How to Learn Effectively" gives a very good selection of experimental evidence to show the methods and factors which make for efficient learning.

The great emphasis given to conflicting theories which have been advanced to explain the different phases of behavior would seem to lessen the interest of beginning students in the text. This is especially true of the theoretical explanations of learning and intelligence and of the emotions.

The author has given, however, a very consistent criticism of the limitations of an extremely mechanistic explanation of learning and behavior and has clearly set forth his own conception that behavior must be explained as an organismic process. In the reviewer's opinion the chapters on imagination and personality are the best in the book.

Georgia State College for Women EURI BELLE BOLTON

Mental Training. A Practical Psychology. By SPERRIN N. F. CHANT. Toronto, Macmillan Co. of Canada, 1934. Pp. x, 195.

This is one of the many books appearing today which attempt practical interpretations of experimental data in psychology. In the reviewer's opinion the present

author has succeeded somewhat better than the average in the rather difficult task of combining simplicity with accuracy.

The title of the book implies an acceptance of the doctrine of formal discipline; but the material presented is justified on the basis of its utilitarian rather than its disciplinary values. The author's acceptance of a dualistic conception of man with its counterpart of "mental" vs. "physical" behavior is rather confusing and somewhat weakens the theoretical portions of the book. Following three general introductory sections on science in general and the viewpoints of "scientific" and "practical" psychology, two sections on learning and memory are found which are the best in the book. Here the author has presented considerable experimental data. Much of it consists of repetitions or variations of classical experiments made in his own laboratory.

The chapters on Learning to Attend, Learning to Reason, Learning to Make Decisions, and Training for Mental Health are decidedly weaker and suffer from a paucity of objective data as compared with the two preceding chapters.

On the whole, many valuable and helpful suggestions are contained in the book which will be of value to the average lay reader.

University of North Dakota C. W. TELFORD

Propaganda. By LEONARD W. DOOB. New York, Henry Holt & Co., 1935. Pp. x, 424.

The systematic position of the author—taken after a review of the shortcomings of other dynamic systems—is that of the attitude-psychology which has been developed chiefly by the Allports. By using this 'psychology of living people,' the author hopes to achieve that 'de-intellectualization of human behavior' which he regards as necessary for an analytical study of propaganda. On the basis of personal experience, combined with a capable review of the literature, the author develops inductively eight major principles of propaganda, together with some twenty-eight corollaries. (Doob, we must note, prefers to assign to this last word the intriguingly original spelling 'correlary.') These principles are then used as guideposts in surveying current propaganda as it is being employed in certain parts of Europe and in the United States. A final section deals with the vehicles of propaganda.

Psychologists will be concerned chiefly with the content of the third and fourth sections of the volume. They will find little of interest in the two introductory sections which present an elementary view of the psychology of attitudes; and they will have no direct concern with the final section on the mediums of propaganda. The third section deals with the presentation of the author's principles of propaganda and is of immediate psychological interest. The principles do not belong to any one category of classification, and one wonders if the author would not have done better to abandon any attempt to clothe them in uniform garb. Certain of them are definitely attempts at classification, while others seem clearly heuristic in nature; this suggests that the effort to group them together may actually serve to obscure their proper rôles. A brief review of the list may make this more apparent.

Principle 1 ("Principle of the Intention of the Propagandist") differentiates between intentional propaganda—in which the propagandist is aware of his interested aim—and unintentional propaganda in which the propagandist does not appreciate the social effect of his actions. The author makes a clear case for his distinction; one regrets only that he did not further expand his treatment of unintentional

propaganda to show its importance in textbooks, in literature and in the moving pictures, for example. The principle is clearly a matter of classification.

Principle 2 ("Principle of Perception") offers a figure-ground approach to the securing of attention, with corollary rules which point to the usefulness of simplification, repetition, and the arousal of auxiliary attitudes as means of leading an audience to one's message.

Principle 3 ("Principle of the Type of Propaganda") again involves a classificatory device. The author proposes to divide propaganda into revealed, concealed and delayed revealed types, according to the disclosure of the aim of the propagandist to the public. His distinction is clearly made and is usefully introduced throughout the later parts of the volume.

Principle 4 ("Principle of Related Attitudes") and Principle 5 ("Principle of the Desired Integration") deal with the common topic of "reconstructing the personality." In seeking to influence action, we are told, the propagandist arouses related attitudes that are instrumental in securing the desired integration, *i.e.* that integration which will predispose people toward the aim of the propagandist. These two principles are essentially heuristic. Both the two major rules and their corollaries would have gained if the phrase "the propagandist attempts to arouse" had been substituted in each case for the more optimistic wording "the propagandist arouses."

The sixth principle—and the most important contribution of the author, in the opinion of this reviewer—deals with the conception of the 'sphere of unpredictability.' The author borrows this phrase from sub-atomic physics and uses it with real profit in psychological contexts. Granting that such a sphere of unpredictability must of necessity exist in predicting human social responses to propaganda, the author suggests five major ways in which the extent of this sphere may be reduced. He proposes that the propagandist may reduce the sphere of uncertainty by (1) creating an impression of universality, (2) indicating paths of action, (3) using repetition and variation to reënforce the desired integration, (4) limiting the stimulus-situation (often by distortion, suppression, and fabrication), and (5) making the primary impression.

The seventh principle deals with counter-propaganda, either by negative or by positive counter-suggestion, and the eighth treats of persuasion—the concentration upon one person or upon a small group in a face-to-face adaptation. With these principles as guideposts, the author now threads his way through the fourth section of the volume, presenting his interpretations of propaganda as used by the commercial propagandist, by the public relations counsel, and by such organizations as the Society for the Suppression of Vice, the N.A.C.P. and the Lord's Day Alliance. After a study of the Communist Party and its propaganda, the author comes to the major contribution of his manuscript as he presents his analysis of Nazi propaganda. The two years he spent in Germany in studying the developments at first hand have resulted in a thoughtful and stimulating analysis of the background of Hitler's rise to power and of the use of high-pressure publicity methods during his regime, both in Germany and in America. This is a section which will repay the careful reader for the time spent in more than one excursion through its pages.

The intellectual level of the volume is by no means uniform; indeed it is difficult to decide for what audience the book is intended. The third and fourth sections are, as has been intimated, mature in outlook, critical in interpretation, and catholic in

documentation. They appear to have been directed toward contemporary psychologists of advanced training. By contrast, the two introductory sections apparently are written for the novice; they are naïve in point of view and frequently are careless both in literary style and in interpretation. The text of these early chapters abounds in imperatives and in pronouncements in the grand manner of what psychology *must* do. The reader is scarcely prepared for the serious contribution of the later sections by the author's failure to comprehend Kurt Lewin's stand or by his reference to "an over-ambitious triumvirate of psychologists known as the Gestalt School." (Doob's interpretation of the point of view of Gestalt-theorie is hardly less astonishing than this characterization of its protagonists.)

In the opinion of this reviewer, however, the general contributions of the later sections overweigh the shortcomings of these earlier chapters. One can even afford to ignore the author's difficulty with the triple negative which leads him to write that "no two psychological concepts can be chosen which are not unequivocal," when context makes it apparent that he means precisely the opposite. One may justifiably overlook a serious failure to grasp the difference between perception and comprehension. One may properly forgive a literary style that is not infrequently florid, granting pardon even when it touches the depths exemplified by the statement that "Martin explodes by shouting that 'all propaganda is lies'." The volume contains both a contribution in its own right and a promise for the future. One may justifiably hope that the next decade will see this promise redeemed in the form of a fuller and more seasoned treatment of propaganda by the same author.

Cornell University JOHN G. JENKINS

Les Origines du Caractère chez L'Enfant. By HENRI WALLON. Bibliothèque de la Revue des Cours et Conférence. Paris, Boivin & Cie., 1934. Pp. 267.

This book is made up of three series of papers which had previously been published separately, these in turn having been written from courses given at the Sorbonne. As a result the book is not organized as a unit, and there is some repetition. Although Wallon's interpretation of the development of behavior is interesting, it seems to be in part contrary to many recent experimental findings which indicate that the development of response in the organism is toward differentiation from functioning as a whole.

Wallon supports his theory of the origin and development of personality by citing examples from the observations of young children and infants of such authors as Preyer, Stern, Magnus, and Kleijn. The theory is based on a division of the sense organs into interceptors, proprioceptors and exteroceptors. In the order listed, these three different sets of sense organs are dominant in turn, and determine the principal interests and reactions of the infant at that stage of development. In early infancy the interoceptors are most fully matured, and cause a predominant interest in oral-anal and digestive functions. This interest is superceded by an interest in balance and equilibrium as the proprioceptive organs (in semi-circular canals and joints) develop. Finally the exteroceptors, which make possible the infant's acquaintance with the outside world, become predominant and the child is increasingly aware of his external surroundings.

The author holds that these three types of sense organs are at first independent of each other, and only become integrated in behavior with development and experience. Working from this threefold division of the senses he presents theories

of the development of adaptive behavior, of emotions, of consciousness of individual bodies, and of self-consciousness.

The interoceptors and proprioceptors—the protopathic senses—are under thalamic control, while the exteroceptors—or epicritic senses—are under cortical control. Paralleling this neural structure, the early unconditioned response or *automatisme* occurs first, and is superceded, with development, by the more complex response, *représentation*, which occurs with experience through the exteroceptors, is under cortical control, and takes into account past experience and conditions that are not a part of the present stimulus-situation.

Emotions do not come under either of these categories of response, but are rather due to an inhibition of response. Their source is in the proprioceptors and they are determined by the tonicity of the muscles. Failure of response causes intensification and spread of tonicity, or hypertonus; and finally spasm, which is felt as intense emotion. Hypertonus may be relieved or neutralized by an adaptive response. As the infant matures and learns more adequate ways of responding (*représentation*) emotional spasms diminish in their frequency and intensity.

As the sensory domains which were originally dissociated gradually become connected, and through contact with the outside world sources of stimulation become individualized, the field of perception becomes unified, and mental continuity develops. It takes symbolic activity and not merely the automatic association of sensations to arrive at a notion of the body and of the use of things. On the sensory motor level the reactions of the child toward his own body present successive stages which coincide with those of his exteroceptive development. In the first period the child makes diffuse movements and is not capable of foreseeing his own movements. Then he begins to expect certain effects. Surprise and diffuse reactions begin to disappear. Finally he is able to identify parts of the body in others. He treats his own body-parts at first as though belonging to others.

Self-consciousness is attained in the same way as is the notion of the body—by the process of identification, juxtaposition and symbolization. By the age of three years the child is able to distinguish between himself and others. He can react not only to present impressions but also to images which he has kept of the past. At each stage forms of activity already passed are not abolished, but are more or less modified by progress. They are capable of recurrence, especially favored by sickness, weariness, or distraction.

University of California NANCY BAYLEY

The Selection of Colour Workers. Edited by Charles S. Myers. New York, Pitman Publishing Corporation, 1934. Pp. xi, 134.

This volume, edited by Myers, describes the evolution of a simple and reliable color discrimination test for the selection of industrial workers. The problem, presented to the National Institute of Industrial Psychology by a firm of color printers, was attacked in turn by A. M. H. Davies, A. Stephenson and W. O'D. Pierce of the Institute staff. The test evolved consists of the ranking in order of saturation of 3 series of 16 color disks each, red, yellow, and blue respectively, under carefully standardized lighting.

The validity of the test is checked by utilizing workers from different industries, and by correlation with a number of other tests and with estimates of efficiency. Both the procedure employed, the results obtained and the incidental observations

offer much of interest to the experimental psychologist. Three appendices deal with the statistical analyses of the results, and with devices for the objective measurements of color, such as the photoelectric cell, colorimeter, etc. There are numerous cuts, tables and figures.

Cornell University ELSIE MURRAY

The Delinquent Boy and the Correctional School. By NORMAN FENTON and Collaborators. Introduction by Lewis M. Terman, Claremont, California, Claremont Colleges Guidance Center, 1935. Pp. 182.

This monograph treats some of the problems of the state correctional school, with special reference to the work of the Whittier State School at Whittier, California. The ten chapters of the monograph deal respectively, with the evolution of a state correctional school, delinquent boys individualized, the child guidance conference, a statistical study of certain characteristics of four hundred delinquent boys, the boy's own story, the inter-relationships of traits of delinquent boys, the program of academic and vocational education at Whittier, social guidance in a correctional school, preparing the boy for return to the community, and a study of certain factors in relation to the adjustment of the Whittier boy on placement.

The work will be of special interest to those who are interested in problems of delinquency, child guidance, and mental hygiene. Of special psychological interest are the chapters that deal with a statistical study of certain characteristics of four hundred boys and the inter-relationships of traits of delinquent boys, although psychological implications are contained in most of the chapters.

The author's approach to the problem is critical and furthermore he stresses the need of pure scientific research concerning the benefits of remedial education. "One of the outstanding faults in our knowledge up to date is the lack of comparable data from equated, control groups of non-institutionalized children under varying conditions. In these days, a private industrial organization would not expend millions of dollars year after year upon any process or technique without a dependable check of results to assure itself of the effectiveness of its methods. Surely the state might reasonably be expected likewise to avail itself of the devices of scientific method to find out whether its costly procedures are really effective."

The monograph is well written and the statistical material is clearly presented. Eight pictures of the Whittier State School add interest to the monograph. The work concludes with a postscript and two appendices. Appendix A is a theoretical discussion of the problem of morale in a correctional institution, while appendix B contains a selected bibliography of 225 titles.

Franklin and Marshall College. PAUL L. WHITELY

Allgemeine Psychologie auf Personalistischer Grundlage. By WILLIAM STERN. Haag, M. Nijhoff, 1935. Pp. xxviii, 831.

"Das vorliegende Buch ruht auf zwei Stützen." Any individual contribution to psychology must, perforce, he based upon the accumulated scientific facts, but this does not preclude the adoption of a new point of view toward the general problems of psychology. "Die personalistische Theorie also bildet die zweite Voraussetzung dieses Buches." Personalism grew up originally as a philosophical system but it has gradually taken on "den Character einer Spezialwissenschaft von der menschlicher

Person." This book gives us the first presentation of the newly developed "personalistik."

The author works out his thesis in thirty-one chapters grouped into six parts. The arrangement of material is traditional and its treatment comprehensive. It begins with a consideration of the philosophical background of psychology, its relations to the other sciences, its methodology, etc., and includes in its course sensation, perception, memory, thought—even attention—on the one hand, and the later trends in drives, intelligence, suggestion, and knowledge. It draws upon the work with animals, children, and individual differences. Throughout he continually refers to the "person" and the attempt is made to relate to it all factual material and to interpret all functional relations in terms thereof.

We shall profit, therefore, by examining in some detail Chapter IV—"Personalistische Grundlegung der Psychologie." Two questions are raised. "Has mind a substrate, and what is the nature of that substrate?" The second question implies the affirmative of the first. "Die psychischen Erscheinungen, Vorgänge und Beschaffenheiten nur da sind als zugehörig zu einem jeweiligen individuellen Ich, welches sie 'hat'." The notion of a substantial soul, with its implication of dualism, is forcibly rejected. Materialism is likewise unacceptable. "Die Person ist eine individuelle, eigenartige Ganzheit, welche zielstrebig wirkt, selbstbezogen und weltoffen ist, lebt und erlebt." However, the person need not always be conscious ("mit Bewusstsein") if other "Merkmale" are present, and so psychology becomes "die Wissenschaft von der erlebenden und erlebensfähigen Person." Professor Stern proceeds to work out with some precision the implications of the foregoing definitions and statements. "Leben" is more than a biological term. "Bewusstsein" is rejected for "Erleben." "Psychische" is broader than "Erleben"—it includes "Dispositionen" and "Persönlichkeit." Finally the relations of the "Person" to the "Welt" must be included. Thus we have developed for us a central explanatory concept in terms of which the mere data of psychology are to be interpreted and to be made significant. In the beginning we were reminded of the "self psychology" of Calkins, but the author goes much farther with the explication of the "person" than Calkins ever did with the "self." Nevertheless the underlying motives appear to be much the same.

The remainder of the book presents the factual material of psychology and its interpretation with reference to the "Person" the author has so carefully defined. The presentation is comprehensive and adequate. Any author must select his material and will naturally exhibit some bias in so doing. In the present case, the bias is somewhat nationalistic and may be seen most strikingly in the extensive bibliography that is appended. Of 634 titles 535 are in German even in fields where the development has been notable in other countries. "Sinneswahrnehmung" contains only three names in English—Helen Keller, J. B. Kline, and G. M. Stratton (1897)—out of a total of 75. "Gedächtnis" which in recent years certainly has had considerable American development, contains only a single reference, Thorndike, to the work in this country. If the book is intended as an elementary text for German speaking students, such a selection, perhaps, is understandable. As a summary of a long period of activity in psychology during which many contributions were made that have now become classic, this new interpretation by Professor Stern is an important work, though many will hesitate to follow its philosophical generalizations and interpretations.

Hobart College FORREST LEE DIMMICK

Arbeits-technik. Die Arbeitskraft; Schultz, Erhaltung, Steigerung. By WALTHER MOEDE. Stuttgart, Enke, 1935. Pp. xi, 267.

The ideal of this book is expressed in its last sentence as follows: "The nation assumes the task of cultivating and protecting the productive capacity of each of its citizens, not only because of the economic value of that capacity, but above all because of the resulting welfare, happiness and mental ease of the individual and the whole of society." It is impossible to give in a review a just account of the variety of the contents of this book. It begins with a description of the efforts of F. W. Taylor in America and Henry Fayol in France. It gives many examples of efforts made in Germany to place the management of labor on a scientific basis. Such terms and concepts as "Kraft durch Freude" and "Schönheit der Arbeit" have 1934 entered into the written laws of the German nation. On the whole the book is not devoted to a complete enumeration of the technical details of the construction and use of testing instruments. Rather it is an attempt to formulate the most commonly used methods in *abstract* terms. Those who are not interested in abstract and somewhat mathematical theories of the technique of labor management might not find much satisfaction from reading the book, although the author has solved the problem as put by himself quite successfully.

Coral Gables, Florida — MAX F. MEYER

Anecdotal History of the Science of Sound to the Beginning of the 20th Century. By D. C. MILLER. New York, Macmillan Co., 1935. Pp. xii, 114.

Although the exposition is admittedly casual and unsystematic, this little book contains much interesting information about the history of scientific acoustics, information which, the author complains, is missing from the standard histories of science. In it the psychologist learns how Galileo (1638) worked out the laws of vibrating strings and the simple facts of resonance, established pitch as a function of the frequency of vibration, and affirmed Pythagoras' dictum that consonance depends on the simplicity of the vibration ratios; how Mersenne (actually publishing earlier, 1636) confirmed Galileo, calibrated organ pipes, and measured the velocity of sound; how Boyle (1660) proved, after the failures of his predecessors, that the ringing bell *in vacuo* is inaudible; how Saveur (1701) discovered the partials of vibrating strings; how Shore (1714) invented the tuning fork; how Sorge (1745), Romieu (1745), Romieu (1751) and Tartini (1714, 1754) all discovered combination tones; how Thomas Young (1801) discovered interference; how Savart (1830) used Robert Hooke's wheel to set the limits of hearing at 8 and 24,000 vibrations; how Scheibler (1834) made his standard tonometer of 56 forks and established A at 440 vibrations how Ohm (1843) laid down his acoustic law of analysis after Fourier (1822) had provided the mathematical basis for it; what Tyndall, Helmholtz, Rudolph König and Lord Rayleigh did for acoustics; and that the biggest historically known noise was the explosion of Krakatoa in 1883. It is pleasant to have Professor Miller return to his original love and thus make us the beneficiaries of his knowledge.

Harvard University — EDWIN G. BORING

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By J. P. GUILFORD, University of Nebraska

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